

Can Physics be Fun?

THE Institute of Physics has recently produced a handsome book on humour and humanity in physics, which offers a rare opportunity to look at a large collection of pieces by physicists writing with a wider audience in mind (*A Random Walk in Science*, an anthology compiled by R. L. Weber and edited by E. Mendoza, pp. 206 (Institute of Physics, Bristol) £4.75). The contents are distinctly variable—at the one end some fascinating insight into the prosecution of science, at the other ghastly laboured in-jokes. Pieces such as Jeremiah Horroxx's description of his observation (sandwiched between preaching sermons at Mattins and Evensong) of the transit of Venus in 1639 and R. W. Wood's account of his encounter with Blondlot, of N-ray notoriety, are real delights and there are many others too. Donald Glaser's problems with getting the bubble chamber accepted and William Whewell's strictures against teaching modern discoveries to students are reminders that science never develops smoothly. For these the book can be recommended, but when physicists try to be funny, how banal they can be and how contrived their nonsense papers, bogus definitions and doggerel seem on paper.

Yet physics—in fact all the sciences—can produce no end of fun in the laboratory. The problem seems to be conveying this fun to others without diluting the impact.

Can humour be categorised? It is customary to speak of the juxtaposition of the ridiculous with the normal as the basis of humour—the virus that we illustrate on page 382 is a good example of this. Sometimes, however, the juxtaposition of the normal with the normal is just as funny, and there is potentially as big a laugh in the predictable happening as the unpredictable. This is perhaps particularly true when the predictable comes from, for instance, a group of people each of whom has individually the capability to behave unpredictably.

Words play a large part in all humour, of course, but apart from the very few really talented people who can use words for conscious humour—and they will not be looking for a career in science when the rewards are so great elsewhere—one can get great pleasure from the unconscious humour of words. Dr Spooner's accusation "you've hissed all my mystery lectures" is pure gold. Even the despicable Post Code, the ugly accretion that the British Post Office is trying, rather unsuccessfully, to foist on the public, has its lighter moments. The Natural Environment Research Council has the Post Code WC1H0AX.

Scientists are peculiarly prone to two extraordinarily persistent strains of humour—the pun and the limerick. It is difficult to find any extenuating circumstances for the pun despite its regular outings in the headlines of *The Guardian*. One would have thought that the towering achievement of the bilingual pun in the book *Mots d'Heures, Gousses, Râmes* would have silenced lesser perpetrators.

The limerick is an altogether healthier animal and it is not too difficult to see why it has such an appeal. It

is palpably not poetry and so there is none of the embarrassment that would be attached to the combining of vulgarity with the immortal art form of, say, the sonnet. And yet it has the neatness of expression and the brevity of utterance of poetry. Relativity, with all its manifest absurdities to the man in the street, has been fertile territory for the limerick. Although the journey of the lady called Bright is well known to all physicists, it is sad to see the general decline and absence here of the marginally more plausible:

There once was a fellow called Fisk,
At fencing exceedingly brisk.
So fast was his action
The Fitzgerald contraction
Reduced his épée to a disk.

It would be impossible to write on humour in physics without giving a very honourable mention to Professor R. V. Jones, who rightly gets several pages to himself in the institute's book. He more than anyone else seems to be capable of capturing the laboratory joke and telling it amusingly. The Aberdeen chimney affair is every bit as funny as Gerard Hoffnung on the bricklayer's excuse. His Cambridge examination question does not make it into the book and deserves a wider circulation:

"It says in *Proverbs*, 30 vv 18–19,
'There be three things which are too wonderful for me
Yea, four which I know not;
The way of an eagle in the air;
The way of a serpent upon a rock;
The way of a ship in the midst of the sea;
And the way of a man with a maid,'

Discuss any three."

100 Years Ago



THE adult female Indian Rhinoceros, which has been in the Zoological Society's Gardens since July 1850, then not larger than a full-sized dog, died on Sunday last, having been ill for some time previously. The coldness of the weather and the fog were probably the exciting causes of its death, though no definite pathological changes have been found on *post-mortem* examination. There were no symptoms of senile decay.

From *Nature*, 9, 132, December 18, 1873.



Making Plans for Energy Research

Colin Norman, *Nature's* Washington Correspondent, looks at the research programmes which may soon be adopted in the drive to make the United States self-sufficient in energy.

SCIENTISTS and engineers working on energy problems in the United States have good cause to be thankful for the energy crisis in general and the Arab oil boycott in particular, for they are about to stimulate massive increases in federal funds for energy research and development. At the beginning of last week, Dr Dixie Lee Ray, Chairman of the Atomic Energy Commission (AEC), delivered to the White House a five-year plan for energy research and development which had been personally requested by President Nixon. It would almost double present expenditures, soaking up at least \$10,000 million of federal funds before 1980. And, at the end of the week, the Senate unanimously passed a bill which sets aside \$20,000 million for non-nuclear energy development over the next 10 years.

Although the AEC's plan and the Senate bill set slightly different courses, their goal is the same: to make the United States independent of foreign suppliers of fossil fuels as soon as possible. In one of his latest energy messages, President Nixon called the effort Project Independence, and he indicated that the goal of self-sufficiency could be attained by the end of the 1970s. The AEC's report, however, says that 1985 "is the earliest date by which self-sufficiency can reasonably be expected", and Senator Henry Jackson, author of the Senate bill, doubts that the AEC plan will be able to buy independence at all.

One problem is that simply pouring money into research and development is not enough on its own to ensure self-sufficiency. To exploit the new technology that flows from the research will require massive capital investment by industry and complex decisions by the federal government. It will also raise difficult legal and environmental problems, and cause political difficulties by putting the federal government even more firmly hand in glove with the energy industry.

The AEC's plan, nevertheless, represents a significant break with tradition, not least because it opens up the Administration's budgetary deliberations to public scrutiny. The plan was requested by President Nixon in June, when he was publicly calling the energy

shortages a "challenge" rather than a crisis. He directed Dr Ray to produce a national energy strategy, a five-year plan for research and development, and a set of recommendations for inclusion in the Administration's 1975 budget—all by December 1. Ray complied, after a complex and frenzied series of planning sessions attended by people from government, industry and academe, and the plan is now at the Office of Management and Budget where it is being studied in detail as part of the preparations for the 1975 budget.

It is a remarkably candid, lucid and optimistic document which proposes huge concentration on short-term objectives to increase fuel supplies and to cut down on inefficient energy use. It is quick to point out where some of the blame for the energy crisis lies: "The energy shortages of today and those projected for future decades stem, in part, from the lack of a coordinated national programme for energy research and development over the past 10 or 20 years". And it adds that the shortages "impart a long overdue sense of

urgency to the effort being launched to meet not only immediate requirements but also the growing needs of the years ahead".

The strategy is to concentrate nearly half of the proposed \$10,000 million on short term goals, such as reducing consumption, substituting coal for oil and increasing production of oil and gas. Almost 40% of the funds are earmarked for medium term efforts such as coal gasification and oil shale development. And a small fraction—7%—would be spent on longer term options such as fusion power. In addition, the plan indicates that private industry can be expected to put up as much as \$12,500 million for energy research and development over the next five years, thanks to the incentives provided by rapidly increasing energy prices. That would bring total expenditure on energy research and development in the United States to more than \$22,500 million before 1980.

Five tasks are outlined in the plan, as follows (see Table 1):

- Conserve energy and energy resources—this would gain \$1,440 million, in comparison with some \$95 million under previous spending plans.
- Increase domestic production of oil and gas. \$460 million would be spent in this area by the federal government, a five-fold increase.
- Substitute coal for oil and gas on a

Table 1 Federal Energy Research and Development Obligations by Individual Programme Element, Fiscal Years 1973-1979 (in \$ millions)

Self-sufficiency task	Actual 1973	Planned 1974	Recommended 1975	Recommended 1975-79 programme	Previous 1975-79 plans for comparison
1. Conserve energy and energy resources	52.8	62.3	166.2	1,440	9.50
Reduced consumption	12.1	22.3	29.9	210	15.0
Increased efficiency	40.7	40.0	136.3	1,230	80.0
2. Increase domestic production of oil and gas	20.0	19.5	51.7	460	90.0
Production	12.8	11.2	31.7	310	50.0
Resource assessment	7.2	8.3	20.0	150	40.0
3. Substitute coal for oil and gas on a massive scale	88.8	167.2	405	2,175	842.0
Mining			45	325	
Direct combustion			30	200	
Synthetic fuels			240	1,270	
Common technology			90	380	
4. Validate the nuclear option	395.8	517.3	731.7	4,090	3,672.3
Safety, enrichment, high temperature gas reactor	129.7	151.7	216.2	1,245.7	1,201.7
Breeder	266.1	365.6	515.5	2,844.3	2,470.6
5. Exploit renewable energy sources to the maximum extent feasible	82.8	123	217.5	1,835	1,232.0
Fusion	74.8	98.7	145.0	1,450	1,132.0
Solar	4.2	13.2	32.5	200	80.0
Geothermal	3.8	11.1	40.0	185	20.0
Total	640.2	889.3	1,572.1	10,000	5,931.3
Supporting programmes (incremental federal funding to present programmes)					
Environmental effect			105.9	650	
Basic research			43.0	300	
Manpower development			5.0	50	
			153.9	1,000	

Table 2 Estimated Contribution to Energy Inputs (Million Barrels per Day Oil Equivalent)

Energy source	1972 Actual Inputs	1980			1985		
		Total with former Pro- gramme	Increment from recom- mended Pro- gramme	Total with recom- mended Pro- gramme	Total with former Pro- gramme	Increment from recom- mended Pro- gramme	Total with recom- mended Pro- gramme
Total energy	34.1	47.0		47.0	57.0		57.0
1. Conservation			4.7	(4.7)		7.0	(7.0)
Production requirements	<u>34.1</u>	<u>47.0</u>		<u>42.3</u>	<u>57.0</u>		<u>50.0</u>
2. Domestic production of oil and gas	21.4	21.5	0.5	22.0	21.5	5.1	26.6
3. Domestic coal production and conversion	5.9	9.1	0.5	9.6	11.4	2.5	13.9
4. Nuclear	0.3	3.6	0.2	3.8	7.1	0.6	7.7
5. Renewable resources (solar, geothermal, hydro- electric)	1.4	0.8	0.2	1.0	1.0	0.8	1.8
Imports	5.1	12.0	(6.1)	5.9	16.0	(16.0)	0.0

massive scale. Coal is the most abundant energy resource in the United States and the objective would be to reverse the trend of the past few years which has seen power plants and factories switching from coal to cleaner fuels. Some \$2,175 million is recommended to develop technologies to stimulate production and use of coal, up from \$842 million.

● Validate the nuclear option. Although the plan suggests that \$4,000 million should be spent on nuclear technology over the next five years, the actual increase is not large. "This reflects the generous level of funding for the nuclear programme over past years, compared to other programmes", the head of the AEC said in her report.

● Exploit renewable energy resources to the maximum extent feasible. The report recommends that fusion research and the development of geothermal energy and solar power should receive \$1,835 million, compared with \$1,232 million under previous plans.

The basic strategy is to press ahead simultaneously with all technologies that look promising and to "pursue most individual research efforts in an accelerated but orderly manner, avoiding the risks of 'great leaps forward' that do not materialise". Nevertheless, high risks should be taken with technologies that are likely to have a very high potential value, such as *in situ* oil shale retorting (see *Nature*, 246, 323; 1973) and techniques for extracting natural gas from tight deposits. Ray also suggests that sufficient parallel studies should be conducted so that, even if some fail, the overall goal will not be jeopardised.

A novel part of the plan is to initiate a "Synthetic Fuels Pioneer Programme" in an effort to help bridge the gap between research on coal gasification and liquefaction, and application of the technology. The idea would be to provide industry with federal guarantees of prices or loans to begin constructing demonstration plants next year. The plants would be built, operated and owned by private concerns and consortia but part of the deal would allow the government to use them for experi-

mentation. With luck, the technology would be developed by 1976 and the groundwork would be laid for a massive private investment programme.

The overall plan represents a huge undertaking, by any criterion, but will it achieve the goal of self-sufficiency? The first thing to be said is that in spite of President Nixon's sanguine public statements, the impact on fuel supplies by 1980 will be relatively small, and the major contribution to offsetting energy imports will come from conservation measures. Nevertheless, Ray predicts that a combination of conservation and increased domestic energy supply could achieve the nirvana of self-sufficiency by 1985 (see Table 2).

The equivalent of 16 million barrels of oil a day will have to be either conserved or added to the domestic energy supply to reach the goal in 1985. The plan indicates that 7 million of them will come from conservation measures and 9 million will be added to supplies if the technology is developed and exploited. If the predictions are borne out by experience in the next ten years, it will be an impressive monument to American ingenuity.

Not everybody is quite so optimistic, however, and looming large among the doubters is Senator Henry M. Jackson, the Democratic Senator from Washington who has carved out a niche as the most powerful member of the Senate in energy matters. He believes that the \$10,000 million ceiling imposed on the programme by the White House is too miserly and his bill, which was passed by the Senate last week, would increase expenditures even more. Although the AEC's plan envisages spending \$10,000 million over five years and Jackson's bill calls for \$20,000 million over 10 years, the essential difference is that the former covers nuclear energy whereas the latter covers only non-nuclear technology.

Jackson's bill would, in short, set up a federal Energy Research Management Project, headed by an independent chairman, and give it the power to direct and coordinate all the government's energy research and development programmes. It would have control over

some \$800 million a year, over and above the present budgets given to individual agencies. An important feature of Jackson's bill is a provision that would authorise the establishment of joint government-industry development projects, so that federal funds could be provided directly for the construction of demonstration plants to exploit technology for coal liquefaction and gasification, oil shale development, and geothermal and solar power. Jackson believes that one of the chief weaknesses of the AEC plan is that it assumes that industry would be willing to spend vast amounts of money on the construction of development plants; direct government support will be required, he suggests.

The prospects for Jackson's bill as it moves through the rest of the Congressional mill are at present unclear—it must now be approved by the House of Representatives—but a similar measure is likely to be approved this week by the House Commerce Committee. There may, however, be a move in the House to put off consideration of the measure until an extensive government reorganisation plan, involving the merger of several departments and agencies into a Department of Energy and Natural Resources, has been dealt with. In any case, the bill only authorises expenditures and the actual money would have to be approved by appropriations committees and Congress itself.

What are the prospects for the AEC's proposals? They will begin to appear in the Administration's 1975 budget request which will be unveiled at the end of January. The plan calls for a total of \$1,572 million to be spent in 1975—nearly double this year's estimated expenditures—and, again, it will have to be approved by Congress.

One thing is certain, however—universities, industries and other institutions conducting research on energy are in for a veritable bonanza. Whether the thaw in energy funds will be accompanied by a sudden freeze in other research expenditures is, however, a matter of some concern. The answer will come in the budget in January.

NEWS

Radical Rethinking Needed in University Science

"UNLESS the quality of research (in British universities) puts it in the 'world league' the case for supporting it from public funds is weak." This is one of the statements made in a report published this week by the British Association for the Advancement of Science (*Higher Education and Scientific Research*, BAAS, Fortress House, Savile Row, London W1X 2AA, 30p).

The report, the first to be published by the British Association in a new series, is the result of the deliberations of a study group set up under the chairmanship of Lord Ashby to discuss the implications for scientific research of the white paper, *Education: A Framework for Expansion*, published in 1972.

The study group expresses concern that research was not given sufficient consideration in the white paper—the word research, it is pointed out, is only mentioned once in the document. The study group observes that this is in spite of the fact that by 1977 there will be 52,000 postgraduates in British universities, 20,000 of them being scientists and probably 15,000 of these engaged in research for higher degrees.

Universities should remain as centres of scientific research, says the study group, and "it would be a calamity for science in Britain, as well as for higher education, if the government's plans for expansion underestimated this need". Although supported research in Britain should be of international standard, according to the study group, this does not mean that universities should concentrate entirely on research to the detriment of other activities. Those university teachers who are not gifted research workers should not desist from the "pursuit of knowledge," says the report, "but rather . . . not all those who pursue knowledge should feel under an obligation to carry out research leading to original publications, or should expect their activities to be financed by grants from research councils or the like".

The viability of research departments with small numbers of graduate students is also called into question in the report. Research students benefit from contact with each other, and facilities for formal postgraduate teaching may be lacking in small departments. These are put forward as reasons why larger departments are to be preferred. Of the sixty or so university physics departments which receive Science Research Council studentships almost one-third receive five or fewer studentships (nuclear physics departments excepted).



Lord Ashby

"If (and we emphasise that this is an arguable matter) the critical mass of a graduate school in a physics department is at least ten postgraduates then it would follow that postgraduate students in many small departments are not getting as good an apprenticeship for research careers as are their fellows in large departments."

The study group points out that there is a case for concentration of resources in centres of research but that this must be balanced by "the need to sustain scholarly standards and enthusiasm among academics in departments which are not designated as centres of research".

For instance, says the study group, research councils might plant viable research units in small departments "even at the risk of depleting any additional resources available to the centres of research". More frequent sabbaticals—perhaps one term free every seven—and an encouragement for staff of small departments to 'migrate' to larger departments as guests could also be a way of resolving the issue.

The unmistakable conclusion which the study group comes to is that in the future there may need to be a significant number of teachers in universities who are not established "in the hierarchy of research workers, but rather in the

hierarchy of interpreters of research to advanced students".

The research councils and the University Grants Committee should now give consideration to supporting graduate schools in some universities but not in others, says the study group. Research should be financed with this in mind and limited resources should not be diffused "as widely as . . . at present". But the initiative is not to be left entirely in the hands of the research councils and the UGC, and the universities are called upon to initiate some of these changes themselves.

Such radical changes, however, will need to be compensated for. The study group lists two such solutions as follows:

- Academics who are not at centres of research in their subjects must be compensated by providing, and financing, opportunities for them to keep close to the growing points of research and, if they so wish, to come as guest workers to centres of research.

- Implicit value judgments which at present overemphasise achievement in published research as a criterion for a career in university or polytechnic teaching must be modified.

But the essence of the report is that the issues raised need to be discussed. The differing rates of growth of the research and university budgets in the years ahead will ensure that the problems of keeping all university staff members actively involved in "world league" research will increase.

£25,000 Prize

THE MacRobert Award for 1973 has been won by three members of Dunlop Ltd for their development of the Denovo tyre and wheel system. They are Tom French, Eric Mitchell and Reginald Edwards, and they share £25,000—the largest prize of its kind in the world. It is awarded annually for an outstanding contribution in engineering or the physical sciences enhancing the prestige and prosperity of the United Kingdom.

The Denovo system permits tyres that have blown out or been punctured to be driven for up to 100 miles at up to 50 miles per hour without attention or permanent damage to the tyre, and is beginning to be offered as an option on cars in Britain.

NUCLEAR POWER

LWRs Are Coming

ONLY by building light water reactors (LWRs) bought from abroad can Britain be sure of a supply of nuclear generated electricity on time, Sir Arnold Weinstein told the Select Committee on Science and Technology this week. Sir Arnold, who is managing director of GEC, the company that holds half the shares in the National Nuclear Corporation, told the committee that since March when he last gave evidence to the committee he had come to the conclusion that the British designed advanced gas cooled reactor (AGR) "does not provide anything like the necessary security of supply". Five AGR stations are under construction. All of them are overdue.

Sir Arnold said that last March he had hoped that it would be possible to build a series of AGRs. But the difficulties with the stations had not been resolved. "This is rather worrying if you are contemplating a programme that will depend on good delivery dates."

Nine countries are building light water reactors and stations totalling 250,000 MW are on order. From 1974 it is expected that 50,000 MW a year of light water reactors will be ordered. There is much more experience of these reactors and there is obviously a worldwide market available, he said.

Sir Arnold confirmed that if the government does decide to allow the Central Electricity Generating Board to order light water reactors, the first station will come, at least in part, from the United States. The steel pressure vessel and the steam generator would be imported along with various other high quality components. About 20% of the cost of the first station might be spent on imports. For the second station the percentage would be lower, although it is possible that even for subsequent stations the pressure vessels will continue to be imported. But the pressure vessel only costs £2 million out of £150 million for the complete station.

Sir Arnold emphasised that the imported parts need not come from the United States for stations after the first one. France, Holland and Germany all have facilities to manufacture LWR parts.

Asked if importing United States reactors would not be a body blow to British advanced technology, Sir Arnold replied that further development work on nuclear systems — including the light water reactors — is still needed and Britain can contribute to this as part of an international programme. He also agreed that a commercial high temperature reactor of about 1,300 MW should be built.

On the question of safety, Sir Arnold

said that the doubts raised in this area were the one inhibiting factor. But the Central Electricity Generating Board has made careful studies of the light water reactors and they are satisfied, he said. Sir Arnold added that as far as he could tell most of the safety questions had been dealt with or were being dealt with satisfactorily except the possibility of the pressure vessel fracturing. But most experts in Britain and the United States are satisfied that the vessel is safe, although doubts have been raised recently. "I would not for a second advocate that we should have a programme of nuclear machines or any other machines that would put the public at risk."

Buying light water reactors would not remove all possibility of building gas cooled stations, Sir Arnold said. If there were a serious accident with light water reactors anywhere in the world that necessitated ending a British light water programme we could always return to AGR stations or to Magnox. Magnox stations have been considered for this programme, but the cost and the need to redesign them extensively (which would again introduce the problems of building prototype plant) led to their fall from favour.

ENGINEERING

Unhappy Lot

ENGINEERS, it seems, are not very happy people. Underpaid, under-recognised, and belittled beside their European counterparts, the 300,000 strong profession in Britain is eager to have its wrongs righted.

That at least was the impression given last week when the Council of Engineering Institutions (CEI) published the results of a survey of 27,000 of its members.

Mr B. Hildrew, a former chairman of the Institute of Marine Engineers and the director of the survey, said that the income of engineers has only risen 1.5% in real terms since 1966. "Considering that we are a technological nation this must be a serious warning."

The CEI also collected, among the welter of statistics on numbers employed in each industry, salaries earned and fringe benefits received, an estimate of the satisfaction felt by engineers with their jobs and prospects.

On the whole, the survey shows, two out of three engineers are well satisfied with life, but in certain industries, notably machine tools and gas distribution, engineers are unhappy with the current situation. And in a number of industries—particularly machine tools, some mechanical engineering, the aircraft industry and vehicle manufacture—some engineers are disenchanted with their career prospects.

On the continent, the CEI maintains, engineers are paid better and have a higher status.

The survey also reveals that the percentage of engineers under the age of thirty has fallen, reflecting the relative lack of interest in the subject at undergraduate level in universities. But the under-thirty intake now includes a very high percentage of graduates, as the engineers are gradually turning themselves into an all-graduate profession. From 1974 all new entrants to institutional membership will hold graduate or equivalent qualifications. In 1966, graduates were outnumbered two to one by non-graduates among members of the institutions. Now the figure is one to one.

The number of engineers in research and development has declined as engineers are transferred into production and construction work to meet the demands created by economic expansion. Members of the CEI see this as a welcome and productive change.

Immaculate Hanker

Received recently in a British university:

Dear Sir,

With due respects and most supplicably I intend to bring the matter under your consideration that I have the honour of seeking admission in your institution to complete the MS(Physics) degree course there.

I am very much interested in pursuing higher studies from your esteemed university. It has been my lifelong ambition to do MS and advanced research in the field of science from a university of a technologically advanced country. I conceive an immaculate hanker to achieve a chance to complete this course in your highly esteemed institution and if in some way, I get admission there, it will be an affair to stimulate me to an inexplicable exhilaration.

I shall very much appreciate your help in securing for me some sort of financial assistanceship in the form of a Fellowship, part-time job or as you may deem fit. In return I have nothing to offer except hard work, full cooperation and everlasting indebtedness towards you.

Perhaps this will be an adequate information I am required to give. I hope you will entertain my long cherished ambition in exorable way, and I am waiting an optimistic expectation for an adorned favourable reply from you.

Yours obediently

Unemployment among engineers has halved to 0.52% over the two years since the CEI's last survey, while the general distribution of engineers throughout industry has not changed much. The massive recession in machine tool manufacture in 1971 has halted, although there has been no appreciable recovery, and the percentage of engineers employed in electrical equipment manufacture and in electricity generation or distribution has dropped. Since 1971, consultancy has taken over from electronics as the largest single employer of engineers (about 10%).

The survey also reveals that engineers who earn more than £3,000 a year also on average receive the equivalent of a further 20% of their salaries in fringe benefits.

Short Notes

Alternative to Hexachlorophane

A NEW disinfectant-detergent preparation for use as a surgical scrub in place of hexachlorophane has passed its first reported independent field trial with flying colours. Hibiscrub, a chlorhexidine detergent solution, marketed by ICI, has been tested by an operating team in the professorial surgical unit at Aberdeen Royal Infirmary, and its findings are reported in a recent issue of the *British Medical Journal* (4, 586; 1973). The unit had previously used the hexachlorophane preparation PhisoHex successfully for many years, but this disinfectant is not entirely satisfactory as it has a fairly narrow range of activity and there is accumulating evidence of the possibility of its toxic absorption. The trials showed that Hibiscrub was just as effective as PhisoHex in maintaining low levels of bacteria on the hands during operations and as an added bonus was more pleasant to use and also cheaper.

The members of the Aberdeen team have acted on their findings and now use Hibiscrub as their routine surgical scrub.

Name Dropping

QUEEN'S University, Belfast, is sometimes embarrassed by the 'Queen's' part of its title and often drops the word from its advertising copy. But there is no sinister motive behind this. The true explanation is more mundane. A spokesman for the university said recently that "when our advertisements are classified alphabetically under 'Q' our prospective applicants sometimes get tired before they get to us". This alteration to the name applies only to publications outside Northern Ireland.

At home they stick loyally to the full title.

BRITISH COUNCIL

Looking East

CONSOLIDATION was the keyword in 1972 as far as the British Council was concerned. The council's annual report published recently (available from 65 Davies Street, London W1Y 2AA) emphasises that the purpose of the council has remained unchanged since it was established forty years ago. It is still in existence to "make the life and thought of the British peoples more widely known abroad . . . to enable students from overseas to undertake courses of education or industrial training in the United Kingdom . . . to bring other peoples into closer touch with British ideals and practice in education, industry and government . . . to make available to them the benefits of British contributions to the sciences and to technology . . ."

The latest people to benefit from the attention of the council are the Chinese from the People's Republic. During 1972-73 there was a marked increase in the council's contacts in China and many Chinese came to Britain last year to study English. Recently, the council has, acting as an agent for the British Government, placed sixty Chinese post-

graduates in science, technology and medicine in one-year posts in Britain. These people are mostly between the ages of thirty and forty-five. The first few have already arrived and are in the process of learning English. They will start their scientific work in January.

The council's budget for 1972-73 was £20.51 million, of which £18.98 million was covered by a parliamentary grant. The remainder came from earnings and private donations. For 1973-74 the budget has only increased by a modest £234,000. About 20% to 25% of the budget is spent on science although the accounts do not allow a direct extraction of this sum.

The council at present employs eighty-one scientifically qualified officers but not all of them are in posts directly associated with science. Fifty-seven of them are abroad and twenty-four are in Britain, most of them in London at the head office.

In the past two years the number of science officers in Europe has doubled. Until 1972 the council had science officers in Spain, Italy and Germany. During 1972-73 a science officer was assigned to the Paris office and recently one science officer has been appointed to Scandinavia and another to the Benelux countries.

DEAFNESS

Wiring for Sound

STUDIES are in progress at the Institute of Sound and Vibration Research at the University of Southampton on the possibility of implanting electrodes into the inner ear to restore hearing in cases of deafness where the auditory nerve is undamaged but the cochlea no longer works.

A feasibility study has been carried out for the Medical Research Council (MRC) by Dr A. R. D. Thornton and Mr N. V. Morgan, both members of the institute.

A number of electrode implants have been carried out in the United States. These have only picked up low frequency sounds and have not helped much in the hearing of speech. But lip reading has been made easier.

The Southampton studies will assess the possibility of plugging a number of electrodes into the cochlea so that a fuller range of hearing can be restored. Dr Thornton says that the operation is already possible with existing technology, although many questions have to be answered before it is certain that the technique can be used.

Operations in the United States have been suspended until better devices are available for implantation, and because there are fears that the silvered electrodes could lead to heavy metal poisoning in the brain. The MRC team will

also be examining the possible long term effects of implanting the electrodes in the cochlea as well as the effects of repeatedly stimulating the auditory nerve with an electrode.

If the technique proves to be workable it is estimated that 20% of deaf people could benefit. But it is far from certain that the research will be successful. Dr Thornton says that "even if I had a patient with a good auditory nerve I would do everything in my power to dissuade him from having one of these operations at the present time". Mr Morgan warns that for people who have been deaf for a long time the technique may create as many problems as it solves. Patients will not just wake up and be able to hear. They will have to get used to interpreting the changed speech patterns that they will hear with the aid of the electrodes.

The institute is also about to start work on a programme of hearing conservation with the help of a £13,000 grant for a three-year period from ICI and Amplivox. Under the leadership of Dr Alan Martin the institute intends to examine why the majority of workers in noisy industries refuse to wear protective clothing.

Dr Martin points out that one company spent £18,000 over eighteen months on hearing protection for its employees, most of it on protective clothing, only to find that only 2% of staff were wearing it.

NEWS AND VIEWS

Climatic Change in China over the Past 5,000 Years

"In the fields of the struggle for production and scientific experiment, mankind makes constant progress and nature undergoes constant change."

Now that scientific contacts with China have been re-established, a vast pool of information about historical changes in climate has become available. The quotation from Chairman Mao Tsetung which appears above comes from a paper by Chu Ko-chen, who highlights one facet of the constantly changing face of nature by presenting evidence of how the climate of China has fluctuated over the past 5,000 yr (*Scientia Sinica*, **16**, 226; 1973). Western climatologists will no doubt be delighted to have this comprehensive review available, even though the author modestly disclaims it as preliminary work which, while elucidating a few problems, also puts forward a lot of new puzzles. As Chu Ko-chen says, "misinterpretations . . . are unavoidable" when looking at the oldest historical records—but misinterpreted or not, at least the records from China are essentially continuous for the entire 5,000 yr period.

The interval of 5,000 yr falls naturally into four sections:

- The archaeological period, from 3000 to 1100 BC, when there were few written records except those carved on the oracle bones.
- The documental period, from 1100 BC to AD 1400, for which there are written documents but no detailed regional reports.
- The gazetteer period, from AD 1400 to 1900, when many districts kept records of history and geography which contain climatic information.
- The instrumental period, from 1900 onwards, in which detailed meteorological records have been kept.

Even for the earliest of these periods important climatic evidence is available. The oracle bones, which contain many inscriptions praying for rains, provide an important clue; evidence of farming activity, and farming methods, provides another. It seems that since the time of the Yangshao culture, 5,000 yr ago, the northern limit of bamboo has moved south by 3°. This corresponds to a decline in temperature along the lower reaches of the Yellow and Yangtze rivers of about 5° C (on average) for January and some 2° C for the mean annual temperature. Yangshao and Yin-Hsu times, at the earliest period of the 5,000 years studied, seem to have been a climatic optimum in China, when rich subtropical fauna and flora flourished in places like Sian and Anyang. But "as to the existence of elephants in the northern part of China during the neolithic age, it is still a moot question," says Chu Ko-chen.

In some ways, the analysis of evidence from the documental period is the most fascinating. Here, there is no lack of data—the problem lies in interpretation to give a self-consistent picture. The keeping of official records began with the founding of the Chou Dynasty (1066 to 256 BC), with first inscriptions in bronze and later written records in bamboo books. But the official historians were not the only people to leave records, and poets in particu-

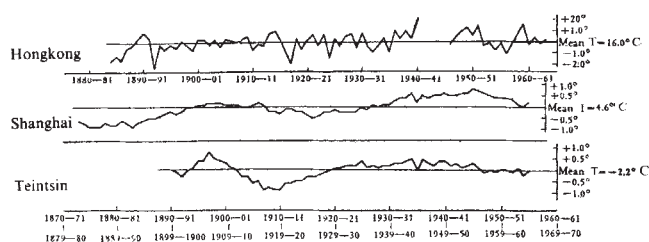
lar have left descriptions of birds and flowers from which evidence of climatic change can be deduced. Certainly, at the beginning of the Chou Dynasty the climate was warm enough to grow bamboo extensively in the Yellow River Valley. But the climate soon deteriorated, and the Han River, a tributary of the Yangtze, froze in 903 and 897 BC. Great droughts followed the freezing, but conditions in the mid-Chou period were rather better. Plum trees, much loved by the Chinese, became common throughout the country, and from about the ninth century BC to the time of Confucius (557 to 479 BC) the Yellow River valley was slightly warmer than it is today. The next cold period began about 200 BC, and in the space of a century there were six very severe winters, with frost and snow occurring in late spring months, and many people froze to death. From the first century BC to the first century AD the climate again recovered, but between AD 155 and 220 attempts to grow oranges in the royal garden at Loyang "failed dismally", even though oranges had been grown in the royal gardens during the Han Dynasty some 300 yr earlier.

The astronomer-poet Chang Heng (AD 78 to 139) had written an ode in which he referred to plentiful orange groves near the "South Capital"—but in AD 225 naval manoeuvres at Kuangling had to be suspended when the Huai River froze, for the first time on record. So the pattern of warm and cold periods continues; it is plausible to argue, for example, that during the 8th and 9th centuries AD temperature and rainfall belts in the eastern part of China were shifted a little north relative to their present day positions, and Chu progresses through the maze of documental evidence up to the 12th century. Then, there was a much more dramatic change in the climate.

In the past few years, climatologists (and even some non-experts) have come to be familiar with the idea of a 'Little Ice Age' which affected Europe and Britain some 500 yr ago, and which may have been at least partly responsible for the loss of Viking colonies in North America and Greenland. But in China, it seems, this little ice age was pronounced in the 12th century, considerably earlier than in the west. Japanese records confirm this; from the 9th century onwards, nobles celebrated the blooming of Japanese cherry trees (a sensitive climatic indicator) and appropriate records were kept until the 19th century. Climatic changes in China and Japan paralleled each other during that millenium. The long cold period lasted effectively from the 10th to the 14th centuries in China, with the severe cold of the 12th century corresponding most closely with the western little ice age. Development and movement of this cold spell can be traced from east to west, since a cold period began in European Russia around 1350, and in central Europe between 1429 and 1465; in England it was during the 16th century that conditions became so serious that they led to the Elizabethan legislation on poverty. Lamb has already pointed out that "the greatest incidence of anomalies moved westward across Europe during the climatic deterioration from 1300 to 1600 and returned to the East

during the recovery from 1700 onwards". Extending this pattern of climatic movement to China and Japan provides one of the most powerful tools yet for climatic research and prediction of future trends.

Records from the gazetteer period provide, among other data, further evidence of this east-west movement of climatic change. Freezing of lakes indicates unusually severe winters, and although this kind of evidence from China and Japan shows that the 17th century had the most severe winters in both countries, in Japan the period of severe freezing started and ended 25 yr before the corresponding period in China. The evidence from the instrumental period seems almost mundane compared with the importance of the historical data, but this shows a pattern of upward and downward swings in temperature, by between 0.5 and 1° C, which is obviously of great significance in predicting future trends, particularly to the farmers (see figure). This kind of swing, with a period of rather more than 40 yr, has been going on for thousands of years, although sometimes obscured by more dramatic changes. Perhaps this is the sort of effect which can be tied to solar activity (see J. W. King, *Nature*, **245**, 443; 1973; and J. Gribbin, *Nature*, in the press), although Chu does point to some difficulties in relating the coldest winters in the 19th century to the pattern of solar variation.



Winter mean temperature (T) for Hongkong (upper curve) and sliding ten-year average of winter mean temperatures for Shanghai and Tientsin.

In summary, beginning 5,000 yr ago: for the first 2,000 yr China was about 2° C warmer than today; then, a series of up and down swings with minima at about 100 BC, AD 400, AD 1200 and AD 1700 and amplitude 1 to 2° C can be traced from the more complete data available. Within each 400 to 800 yr period, smaller cycles of 50 to 100 yr in length can be traced, covering 0.5 to 1.0° C in their variation; for both kinds of cycle, the coldest period begins at the Pacific coast of Asia, moving west as a wave right through to the Atlantic coast of Africa and Europe.

So Europe, following much the same pattern of cyclic variations, usually lagged behind China. Chu argues that this is because both climates are controlled by the Siberian high pressure system, and that fluctuations in this high affect the whole of Eurasia in a consistent pattern. This is a particularly persuasive argument following King's suggestion that solar radiation changes influence the weather through an interaction of energetic particles with the North Atlantic high, which is a counterpart to the Siberian system. Preliminary though this investigation might be, together with work now going on in the west (see *Nature*, **245**, 408; 1973), it emphasises the way in which an understanding of climatic change is beginning to emerge. As Chu points out, this is one field of research where Mao Tsetung's policy of "making the past serve the present", and indeed the future, is particularly apposite. Continued study of ancient climate can undoubtedly lead to better long range forecasting.

J. G.

Records of the 1973 Eclipse

THE preparations for the remarkable total solar eclipse of June 30, 1973, were described earlier in this column (see *Nature*, **243**, 490; 1973). In this issue appear some of the first results of experiments associated with this event (Beckman, page 411; Beckman and Clucas, page 412; and Koutchmy *et al.*, page 414).

Skies were clear along most of the track of totality, though wind-raised dust spoiled some of the experiments located in the Sahara. But this has not prevented Koutchmy *et al.* from achieving a notable 'first' by their detection of a 100 km s⁻¹ motion of a large filament in the solar corona, about 3 to 5 solar radii out. This the Franco-Russian team achieved by the simple device of comparing photographs taken at two observatories spaced 90 min apart along the Sahara segment of the track of totality. At distances from the Sun of 3 to 5 solar radii the corona still consists essentially of almost fully ionised hydrogen, a hydrogen plasma, in which much of the acceleration of the solar wind is thought to take place. It is feasible that the outward motion of the filament is direct evidence of this acceleration, but more importantly the relatively rapid motion which was observed runs counter to prevalent models of the corona in which large motions on such a short time scale are neglected.

A complementary experiment on board Concorde 001 was designed to search for changes in coronal structure closer to the Sun, within 1.5 solar radii. The detection of such changes becomes feasible because the speed of Concorde 001, Mach 2.05, retards the apparent motion of the Moon to give a ten-fold increase in the duration of the eclipse from 7 to 74 min. A preliminary report (Beckman *et al.*, *Nature*, **246**, 72; 1973) has indicated that this and the other experiments on Concorde 001 went very well. Final results from this group of scientists who have obtained more total eclipse time than most solar scientists get in a lifetime are awaited.

During the supervision of his main experiment on Concorde, Beckman (page 411) recorded the time between the first appearance of the red solar chromosphere and the first appearance of the bright photosphere which marked the end of the slowed down eclipse. Because of this slowing down this simple observation has been turned into an estimate of the thickness of the solar chromosphere that is about twice as accurate as previous ground-based estimates. The chromospheric thickness is notoriously difficult to measure because the relatively faint chromosphere is adjacent to the very bright photosphere and is only about as thick as 0.2% of the solar diameter. The thickness is of importance in any theories of the solar atmosphere because the chromosphere is a major feature of this atmosphere.

A minor puzzle is emerging from experiments designed to detect atmospheric gravity waves in the Earth's atmosphere induced by the eclipse. Such waves differ from the familiar acoustic waves because a term representing the work done by a wave against the Earth's gravitational field is included in the equation of motion. They are important because they represent one of the few plausible mechanisms by which considerable amounts of energy may be transported vertically in the atmosphere above and across the tropopause.

Although the atmospheric gravity waves are believed to

be always present, the mechanism by which a solar eclipse generates them, though complex, is simple compared with the presumed ongoing processes. An essential feature of this mechanism is the cooling induced at various levels in our atmosphere by the Moon's shadow. Whether one detects the waves is therefore a measure of whether the correct model for solar atmospheric heating is being used and whether the corresponding model for generating the waves is correct.

The puzzle is this: during the eclipse of March 7, 1970, effects observed in the ionosphere and at ground level were construed as evidence of eclipse induced gravity waves. During this year's eclipse such effects seem to be absent (Schödel *et al.*, *Nature*, **245**, 87; 1973; Beckman and Clucas, this issue, page 412).

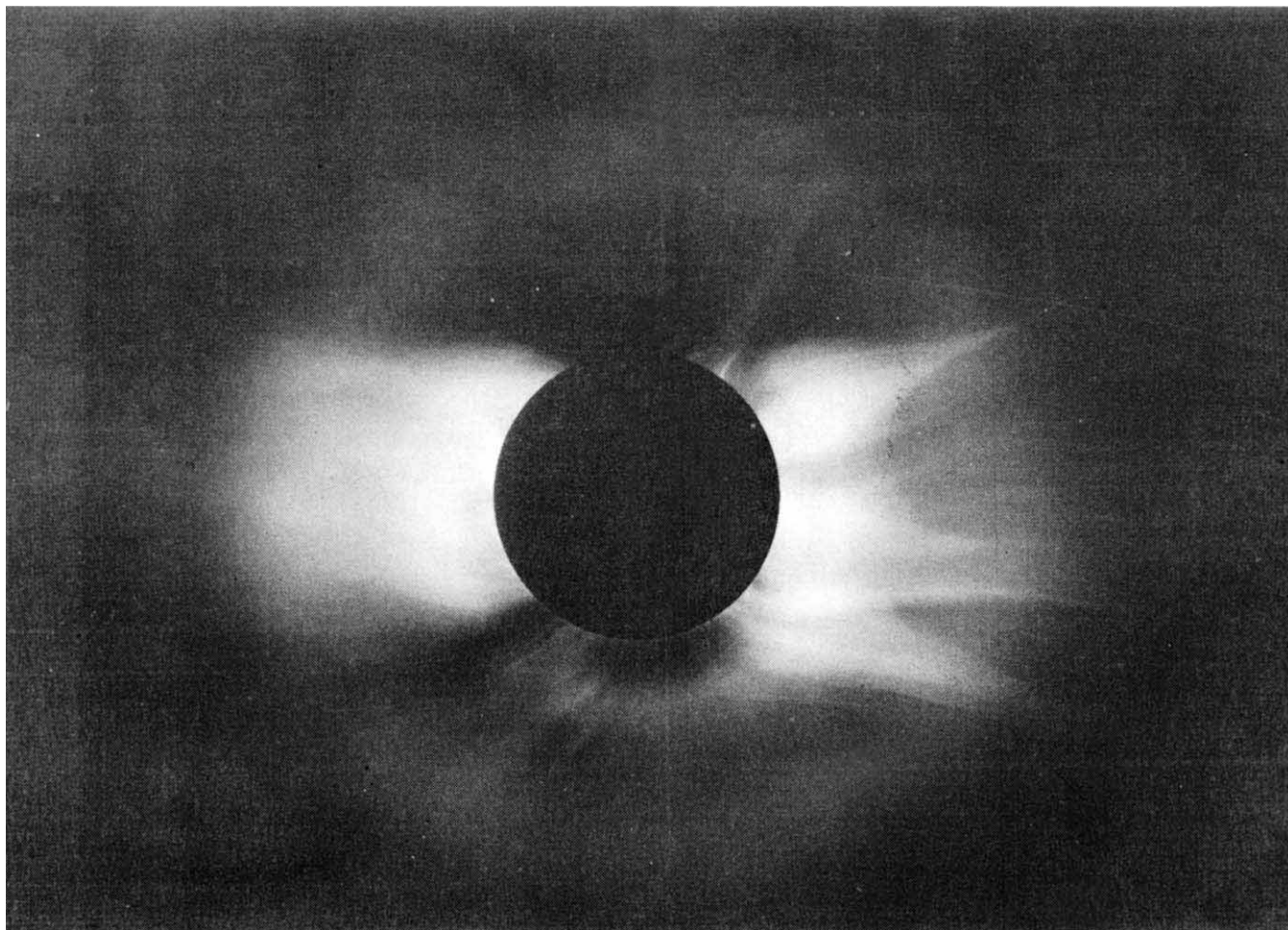
The travelling ionospheric disturbances observed by Davis and da Rosa in 1970 (*Nature*, **226**, 1123; 1970) were convincingly accounted for by Chimonas and Hines (*J. geophys. Res.*, **76**, 7003; 1971) in terms of atmospheric gravity wave generation in the atmospheric ozone, particularly around 45 km altitude. The absence of any evidence for atmospheric gravity waves in the ionosphere in 1973, reported by Schödel *et al.*, could thus be explained in terms of a reduction in atmospheric ozone. This is plausible because the 1970 observations were made north of the equatorial zone in the month (March) in which the amount of ozone is near the annual maximum. By contrast, the 1973 observations were made in the equatorial

zone in June and the amount of ozone is expected to be lower. Moreover, 1970 was close to a peak in the eleven-year sunspot cycle and hence a peak also in solar X-ray and ultraviolet-ray flux. This flux generates ozone. Thus since in 1973 these fluxes are half-way to minimum values the solar cycle is another way to account for the 1973 absence of evidence for gravity waves in the ionosphere.

The ground pressure waves detected in 1970 by Anderson *et al.* (*J. atmos. Sci.*, **29**, 583; 1972) were a hundred times larger than those predicted by Chimonas using the same model that he has used successfully to account for the ionospheric observations. The experimental upper limits placed on the 1973 ground pressure waves are of the order of the theoretical predictions.

It is not certain that the ground pressure waves detected in 1970 were induced by the eclipse but, assuming that they were, then one has to find a way of leaving the ionospheric story untouched while at the same time accounting for the large 1970 effect on ground pressure and an effect in 1973 that is at least 100 times smaller. Hope lies in invoking mechanisms for generating the waves in addition to that based on ozone. Near ground level the absorption by water vapour of infrared radiation re-radiated from the ground can possibly be used to explain the large 1970 effect, but it is not easy to see why this mechanism for generating atmospheric gravity waves was so much less effective in 1973.

B. W. J.



The solar corona photographed by J. Fagot and S. Koutchmy at Moussoro (Chad) at 12–14 h UT on June 30, 1973, with an exposure time of 10 s. An Ektachrome 18 × 24 cm film was used in combination with a 15 cm diameter compensator made by MTO (France). The standard camera focal length 3 m, objective diameter 20 cm, was made by the Institut d'Astrophysique, Paris, and the University of Kiev. Note the background of the Moon seen through the central hole of the compensator. The Moon is not concentric with the Sun, because the observation was made from a point 88 km from the central line of totality. (Copyright, Institut d'Astrophysique, CNRS, Paris.)

Particle Creation in a Big-bang Universe

ALTHOUGH the 'big-bang' models of the Universe were formulated nearly fifty years ago, investigations close to the big-bang events have been few and far between. Physicists have, on the whole, been hesitant about applying their laws in conditions very different from those that operate 'here' and 'now'. One notable exception is the work of George Gamow more than twenty-five years ago about element synthesis during the first few seconds after the big bang. This led to the prediction of the microwave background which was first observed in 1965.

During the few years since this important discovery, cosmologists have become bolder in tackling the big bang. Many authors, like Zeldovich, Sexl, Parker and others, have examined the possibility of particle creation as a gravitational effect in the highly curved and rapidly expanding Universe just after the big bang. An interesting and novel attempt in this direction has recently been made by Audretsch of the Fachbereich Physik der Universität, Konstanz (*Nuovo Cim.*, **B17**, 284; 1973). This work arose partly out of the author's discussions with the relativity groups in London. Audretsch's approach may be briefly described as follows.

In quantum mechanics the propagation of massive spin- $\frac{1}{2}$ particles, like the electron, is described by the Dirac equation. In the flat space of special relativity, such a particle of given mass and momentum can exist in one of four possible states, described by a plane wave. Each state is characterised with spin, which can be $+\frac{1}{2}$ or $-\frac{1}{2}$ and by energy which can be positive or negative (but with the same magnitude). In order to avoid the awkward negative energy solutions Dirac proposed the 'sea' of negative energy particles. This sea is completely filled and is unobservable. A free particle of positive energy can propagate for ever without taking any notice of this sea. If, however, there is an external force acting on it, such as the electromagnetic field, it changes energy. This can lead to a particle from the sea 'jumping up', for example. Such a particle, and the 'hole' it has created in the sea, then

appear as a pair of particles. This is how particle creation from an energy source like radiation is believed to take place.

Audretsch has applied this picture to the expanding Universe and he comes up with certain important differences. When he considers the plane wave solution of the Dirac equation in the expanding Universe, he finds that only in certain special circumstances do the positive and negative energy solutions stay unmixed. The special cases are (1) the particle in question has no mass (a neutrino, for example) and (2) if the particle is massive it is at rest in the cosmological substratum. For a massive moving particle mixing of states must take place. That is if the particle is initially described by a plane wave with positive energy, it will later on also acquire states of negative energy. This curious effect, Audretsch finds, is entirely due to the expansion of the Universe. By analogy with the flat

space picture just described, he concludes that the appearance of negative energy states must be interpreted as particle creation. This is how matter appears in the expanding Universe.

According to Audretsch the effect of expansion is significant for particle creation when the age of the Universe is comparable to the microphysical time scale—when the Universe is only about 10^{-21} s old! This is hardly surprising in a theory which seeks to relate microphysics with cosmology. In this work Audretsch has steered clear of many relevant but complex issues, such as the implications of quantum field theory (this work does not quantise the Dirac field), the effects of quantising the space-time geometry, the implications of the negative energy sea for Einstein's field equations and so on. No doubt such problems will keep theoretical big-bang cosmologists busy in years to come.

J. V. N.

DEMOGRAPHY

Conflicting Population Reports

from our Demography Correspondent

ONE of the professed aims of the United Nations World Population Year, 1974, is improvement of knowledge of population trends, particularly in the Third World. The recent history of population projections for the United Kingdom, where population statistics are among the world's best, in terms of range and quality, indicates that this will not be easy and the conflicting findings of two recently published studies give no basis for supposing that the situation is improving in Britain.

A set of official projections (*Population Projections*, No. 3, 1972–2012, HMSO, London; 1973) suggests a population of just under 58 million for England and Wales by the year 2011, a rise of about 9 million over present numbers. By contrast Gilje and Gould of the Greater London Council Intelligence Unit (*Q. Bull.*, No. 24; September 1973) suggest a possible population of about 50 million for the same date, which is only a fractional increase over the present figure. The team of GLC researchers has a very good record of population studies and the projection methods of the two reports are broadly similar. How then can such conflicting reports result?

The answer is that the assumptions on future fertility patterns differ crucially. Both reports use the same mortality and migration assumptions and, though some demographers might argue the future mortality decline assumptions are optimistic, changes in this factor during the projection period are likely to be minimal.

Fertility patterns result from a complex of economic and social factors affecting the decisions of individuals not merely on one matter but several—principally age at marriage, desired family size and the spacing of births in relation to marriage and preceding births. To predict future changes in each of these elements demands the prediction of future patterns of economic and social factors which influence them. This is the familiar problem of an intricate set of interdependencies and feedback relationships. Economists and resource planners see population estimates as basic inputs for their studies of future trends but demographers equally require these projections to make their fertility assumptions.

The population increase suggested in the official projections stems from the assumption that mean completed family

size will stay at about 2.33 and that the decline in births which has occurred since 1964 is a result of birth postponement rather than a decline in completed family size. Support for this assumption comes from replies to questions on intended family size in the 1971 *General Household Survey* (HMSO, London; 1973) but, as the GLC study points out, intended behaviour may not be a good indicator of actual behaviour in this case and replies may reflect spontaneous feelings at interview time rather than firm family size plans.

The authors of the GLC report argue that the fall in births is not a temporary feature at all but a permanent one from a decline in completed family size and an increase in the spacing between births. To support this assumption they cite the way couples who wish to postpone parenthood can do so effectively with modern contraceptives and several reasons which may be encouraging more couples to do this, including the desire of wives to have careers and attaching greater importance to material well-being than family rearing. Against the official projection's assumptions that births are merely being postponed, Gilje and Gould put forward the novel point that the 'appropriate' age range for having a family is socially defined and that some of the couples who may intentionally be postponing births will pass the upper limit of the range without ever achieving their originally intended family size.

The zero growth projection produced by the GLC team is one of two they have produced, on differing fertility assumptions. The other gives a 2011 population of about 53 million, still 5 million below the official figure. None of these three figures can be said to be any more 'accurate' than the other predictions.

At present, Britain is in a transition period of fertility behaviour, contributing at least partially to the differences. Only when the transition ends and the patterns stabilise will the range of alternative assumptions on future fertility behaviour be narrowed.

GEOPOLITICS

Forensic Seismology

from our Geomagnetism Correspondent
"I REMEMBER, when our whole island was shaken with an earthquake some years ago, there was an impudent mountebank who sold pills which (as he told the country people) were very good against an earthquake". As the multi-million dollar industry purveying nostrums for the common cold indicates, the sort of quackery recorded by Joseph Addison in *The Tatler* of about 250 years ago persists to this day, albeit in a form more appropriate to a

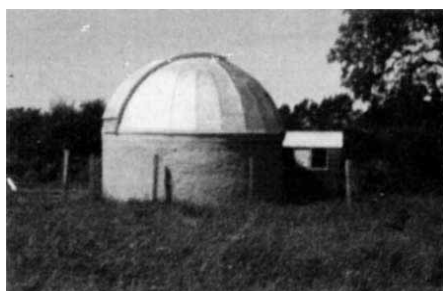
supposedly more sophisticated market. Of course, it is hard to imagine that many people today would expect any problem associated with earthquakes to be resolved quite so easily, although, as Dr H. I. S. Thirlaway pointed out some time ago in the Tenth Harold Jeffreys Lecture, some politicians of the early 1960s came very close to it in supposing that there might be a ready-made scientific pill to cure the seismic malady of the day—the headaches induced by the inability to distinguish between earthquakes and underground explosions at a distance. Whether those same politicians ever reversed Addison's indignation and privately berated the seismologists as charlatans for not being able to come up with an appropriate medicine bottle has, not surprisingly, gone unrecorded. At any rate, in public they blamed not the scientists but the science, bandying about such phrases as 'Stone Age' and making invidious comparisons with the state of the more trendy sciences of nuclear physics and space research.

With the publication of Thirlaway's lecture (*Q. Jl R. Astr. Soc.*, **14**, 197; 1973), one is again able to savour the retrospective delights of those early days of treaty making, when one optimistically assumed that what was to become the limited Test Ban Treaty of 1963 could be converted into a comprehensive agreement by rapidly pulling the right pill out of the doctor's hat. This is, of course, the field that Thirlaway himself has christened "forensic seismology". A minor diversion to the dictionary is required here, revealing that 'forensic' derives from the Latin *forensis* for forum or market place—a formal definition which perhaps does too little to emphasise the element of argument implied by the modern adjective. In any event the intended analogy with forensic medicine, or (that dictionary again) "the application of (medical) knowledge to the elucidation of doubtful questions", is perfectly clear and suggests that the experiences of the medicine men of the late eighteenth century might have contained some lessons for the seismologists of the late 1950s who found themselves confronted with scientifically illiterate politicians anxious to politicise science to save mankind.

Consider with Thirlaway, for instance, the example of one John Hunter, a medical man of some repute who was persuaded to give evidence in that capacity in the case of *Rex versus Dunellan* at the Warwick Assizes of 1781. The aforementioned and unfortunate Dunellan stood accused of the crime of poisoning by laurel water, and his prospects were not particularly bright when Hunter was called to appear as an 'expert' witness for the prosecution. Were the described symptoms

ASTRONOMY

Lockyer's Telescope



SIR NORMAN LOCKYER'S 6½ inch Cooke refractor has found a new home only 80 miles from the observatory which he founded at Sidmouth in his declining years (see *Nature*, **240**, 176; 1972). Early in 1972 the Exeter Astronomical Society started building a 15-foot-diameter dome to house Lockyer's historic instrument, and on August 11 this year the inauguration of the completed project was performed by Professor G. K. T. Conn, Director of the Norman Lockyer Observatory.

The telescope is that with which Lockyer made his spectroscopic analyses of the Sun, culminating in his discovery of helium. One hundred years since Lockyer made his first drawings of Mars the instrument is being used for drawings of the present opposition of that planet by the amateur members of the Exeter Astronomical Society.

An extensive programme of observations, including lunar and planetary studies, is in progress by members of the society, and soon photographic work will be started with the installation of an electric equatorial drive.

consistent with death by laurel water or were they not? the court wanted to know of Hunter. The poor Hunter did not know. "I do not mean to equivocate," he said, "but when I tell the sentiments of my own mind, what I feel at the time, I can give nothing decisive."

Nor, according to Thirlaway, could the scientists on the technical working group of the Geneva Conference of the Discontinuance of Nuclear Weapon Tests who began work on October 31, 1958. They found themselves in much the same dilemma as John Hunter; and, like John Hunter, they discovered too late their lack of clothes. Of course, Hunter and the early history of forensic medicine were hardly uppermost in their minds; nothing had been learned from history (a common enough failing)—neither from the vast case history of medico-legal activity, nor, Thirlaway might have added, from the equally vast precedent involving the many Earth scientists who had been called upon to

give evidence in legal disputes centred on such geological phenomena as metaliferous lodes with an unfortunate tendency to stray at depth onto a neighbouring claim. The seismologists who gathered in 1958 to do their bit in preventing man from blowing himself up simply did not possess the knowledge assumed of them. Fifteen years later they still do not possess the knowledge needed to monitor underground explosions faultlessly; but they do have a greater knowledge of themselves. Indeed, in summing up the failings of the earlier years, Thirlaway could well have written an appropriately grave epigram: "We were persuaded to appear as witnesses to testify about what we did not know".

But although the embarrassing lack of knowledge where knowledge was (perhaps naively) expected was the most important single difficulty that Thirlaway and his colleagues had to face, matters were hardly helped by the circumstances in which this deficiency became clear. The seismologists who were precipitated into science politics in the late 1950s hardly surprisingly felt themselves to be in an alien environment. "We did not enjoy the public gaze in which often we had to work; we had little or no experience in hand-

ling the news media. . . . We were not careful enough to separate fact from opinion and comment for the benefit of our officials; we did not give sufficiently direct answers to simple questions; we filled our answers with technical terms and jargon as though addressing professional societies rather than State officials. . . ." In short, insofar as the politics of science was concerned, the seismologists as well as the science were in the Stone Age; they were, as Thirlaway admits, innocents abroad in the tough political arena.

In his now-published lecture Thirlaway also admits much more. But it is nearly Christmas, the season of goodwill and charity; and I shall not act against traditional precepts by spoiling a good story completely. Thirlaway's confessions need to be read in their full form without the aid of an interpreter. But how full is full? Surely there could be more? Could Thirlaway be persuaded to testify about what he does know to the length of a book? To take the point quite seriously, the early (and later) days of forensic seismology form an important, though neglected, part of the history of the interactions between science and politics. It would be a pity if the full story were to be lost.

INSECT EYE

Duality of Vision

from our Insect Physiology Correspondent

It has long been held that separate photopic and scotopic mechanisms for day and night vision, such as are well recognised in vertebrates, do not exist in insects. The most familiar effect of the duality of human vision is the so-called 'Purkinje shift'—a change in the wavelength to which the eye is most sensitive as the light becomes more dim. Such a change was demonstrated in *Drosophila* many years ago by Fingerhman and Brown, but the tendency has been to find other explanations for this observation. In recent years it has been shown by Goldsmith *et al.* in flies, and confirmed by Carlson in moths, that prolonged vitamin deficiency, extending over many generations, induces a loss in visual sensitivity which affects the longer wavelengths more than the shorter wavelengths and the ultraviolet; a result which is reminiscent of 'night blindness' in man; and Cozens found that dark adaptation in insects involved a two step process, not related to pigment migration, suggesting the separate existence of photopic and scotopic mechanisms.

In 1972 S. L. Swihart, while studying the waveform of the electroretinogram (ERG) of diurnal butterflies and moths,

found that different components responded differently to drugs, inorganic ions and the like, and differed from each other in regard to flicker fusion frequency, latency and spectral sensitivity, as well as to light adaptation. All this strongly suggested the existence of retinal duality. Swihart (*J. Insect Physiol.*, **19**, 2035; 1973) has now investigated this question systematically, applying the sort of tests from which the existence of the dual system of vertebrates has been inferred.

Swihart has shown (1) that there is a change in the flicker fusion frequencies on adaptation to night vision; (2) that there is a change in the slope of the curve of "stimulus energy vs response" and, as was to be expected, the photopic receptors have a higher threshold and lesser slope than the scotopic; (3) there is a break in the curve of activity in visual interneurons as the energy of the stimulus is steadily increased; and (4) the electric potentials recorded as evidence in the ERG are already operating in the pre-emergent (pharate) adult butterfly and are therefore unlikely to be originating in neural structures.

Finally, Swihart has formulated a theoretical model of the insect eye, based on the duality concept, which can reproduce quantitatively the changes obtained experimentally in the ERG records in respect to flicker, in both waveform and amplitude.

POPULATION ECOLOGY

Vole Demography

from our Animal Ecology Correspondent

THE regular occurrence of sunspots, the cycle of ozone content of the atmosphere, the quality of vegetation and the activities of predators have variously been held up as the explanation of rodent population cycles. There seems a need among ecologists to look for just one factor as the sole directive agent.

It is therefore refreshing to read Lidiker's paper on the regulation of an island population of *Microtus californicus* in which no single controlling factor emerges with more importance than any other (*Ecol. Monogr.*, **43**, 271; 1973). Lidiker studied the voles on Brooks Island in San Francisco Bay from their arrival there in 1958 until 1971—a period of thirteen years. Such long-term studies are invaluable and regrettably rare, but infinitely more useful than the short-term studies upon which much ecological creed is built.

A few voles reached Brooks Island from a neighbouring islet 230 yards away in the summer of 1958 and for the following 2 years the population increased very rapidly. During the study a variety of censusing techniques were used, but most of the sampling was carried out on a trapping grid of between 0.08 ha and 0.49 ha. After the initial 2 years of rapid increase the population settled down to a regular pattern of numerical change. Reproduction started about 2 months after the start of the rainy season (November); a period of high corpus luteum production and large litter sizes began about 3 months after the start of the rains; the build-up to peak numbers did not occur until very late in the breeding season; body growth was halted for about 5 months from the start of the dry season; moulting occurred in two discrete seasons, May to June and September to November; and, finally, winter densities alternated high and low. The 2 years of colonisation were typified by numerous births within a month of the rains starting, a rapid growth of the population early in the breeding season, and a continuous high density showing no winter decline. Clearly then some factors operated during the 11 years of 'stability' which did not operate in the colonisation years.

The pattern of wet and dry seasons has an important, but not altogether understood, influence on dynamic processes. The vegetation responds within 5 days to the start of the rains, yet the rodent population takes 2 months. Lidiker thinks that those individuals that have passed the dry season, usually at high density, suffer some kind of physiological damage. The marked population increase starts when their few offspring themselves start to breed. The influence of physio-

logical damage on rodent population dynamics has been suggested by Chitty in his studies on *M. agrestis* (*Phil. Trans. R. Soc.*, **B236**, 505; 1952). Lidiker considers that nutritional defects of the vegetation did not cause the delay, since young males increase in weight and recommence growth long before other age and sex groups.

The alternation of overwintering numbers seems to be a normal oscillatory process. High winter numbers damage the habitat and induce low dry seasonal survival the following year and also low winter numbers. This reduction in grazing pressure allows the forage to recover and so better survival is seen in the third year.

When all the many population data Lidiker has amassed are examined it is clear that no single factor emerges as the dominant controlling influence. Lidiker lists six factors—both density dependent and density independent—which interact to provide control. If any one factor must be singled out as having a more direct effect on the population than any other, then it must be the pattern of rainfall.

The regular pattern of population dynamics could be modified into the 3 or 4 year cyclical pattern (for example, Krebs, *Tech. Pap. Arct. Inst. N. Am.*, No. 15; 1964) regarded as 'normal' for small rodents by (1) the action of predators responding to the build-up by increased breeding (there were no predators on Brooks Island); (2) delay in the onset of breeding due to physiological damage after a 'crash'; (3) emigration occurring; and (4) a rapid build-up when (1), (2) and (3) have all had their effects.

The Brooks Island study shows that the dynamic pattern is neither controlled exclusively by extraterrestrial phenomena, interpopulation phenomena nor intrapopulation phenomena, but by a complex interaction of factors. Lidiker has done rodent population biology a real service by pointing out that at least one natural population behaves according to the complex dictates of the whole environment of which it is a part.

PURPLE MEMBRANE

Light-driven Pump?

from a Correspondent

THE purple membrane is a specialised area of the cell membrane of the bacterium *Halobacterium halobium*. Recently the vivid colour of this membrane fragment was shown to be due to a complex of protein with retinal, the aldehyde derivative of vitamin A. The protein is the only species in the membrane and is packed in a crystalline array (Oesterhelt and Stoekenius, *Nature new Biol.*, **233**, 149; 1971; Blaurock and Stoekenius, *ibid.*, 152). Since retinal also gives the

visual pigments their characteristic colours, it was suggested that the purple membrane might be a photoreceptor. Oesterhelt and Stoekenius, however, now summarise experiments done by themselves and several coworkers which lead them to suggest that the membrane pumps protons out of the bacterial cell by using light energy. The resulting gradient may be a source of metabolic energy (*Proc. natn. Acad. Sci., U.S.A.*, **70**, 2853; 1973).

The purple membrane forms in the cell envelope if a growing culture is aerated with oxygen-poor air, whereas if the culture is vigorously aerated with normal air the membrane fails to form in significant amounts. If aeration of a growing culture is stopped altogether, the total cell protein fails to increase further but the purple membrane increases fifty times. Illuminating an oxygen-poor culture can increase the proportion of the purple membrane to half the total membrane area. Moreover, growing cells from a late-log phase culture use less oxygen when illuminated than when in the dark; one less oxygen atom is consumed for about every thirty photons absorbed by the purple membrane in the cells (*FEBS Lett.*, **36**, 72; 1973). Starved cells, which consume reduced amounts of oxygen, similarly use even less oxygen while in the

light. These results taken together indicate that the purple membrane compensates for a lack of oxygen. By what means?

When isolated purple membrane fragments in an unbuffered medium are exposed to a bright light, the pH drops, and it recovers the original value in the dark. Correspondingly, a 'bleached' form ($\lambda_{\max}=412\text{ m}\mu$) of the purple protein is found during the illumination, and the normal form ($\lambda_{\max}=568\text{ m}\mu$) is recovered in the dark. The time courses for bleaching and recovery are the same as for the pH effects (*Eur. J. Biochem.*, **37**, 316; 1973). The starved cells similarly make the medium more acid as long as the light is on, and anaerobic cells also cause a net drop in pH in the light. The net increase in proton concentration when whole cells are illuminated is said to be at least twenty times that for the same amount of purple membrane after isolation. It is therefore suggested that the membrane protein may operate in a cycle, repeatedly picking up a proton from within the cell and ejecting it when light is absorbed. Thus the purple protein may be the first membrane pump to be isolated.

The authors refer to Mitchell's chemiosmotic theory (*J. Bioenergetics*, **3**, 5; 1972) and suggest that a resulting electrical potential across the membrane is

FOOD BIOCHEMISTRY

Fragrance of Bread

from our Soviet Correspondent

AT present approximately 100 compounds are known to contribute to the characteristic fragrance of baking bread. These include, in particular, carbonyl compounds such as furfural and oxymethylfurfural, and also fermentation products such as organic acids, complex ethers, acetaldehyde.

A recent study, carried out by a team from the "A. N. Bakh" Institute of Biochemistry of the Soviet Academy of Sciences and the All-Union Research Institute of the Bread Industry, studied how the formation of these aroma-producing substances depends on the thermal and time regimes of baking. It was found (see Kretovich *et al.*, *Prikladnaya Biokhimiya i Mikrobiologiya*, **9**, 5; 1973) that the fragrance is essentially due to chemical changes within the crust, little contribution being made by the soft crumb, the chemical composition of which remains relatively stable. The decisive role in the formation and accumulation of carbonyl compounds within the crust is played by the formation of melanoidine during the baking process.

This process falls into three stages. During the first stage, which lasts for about 25% of the baking time, the

temperature of the crust rises from 20 to 90–100° C, while the temperature of the crumb rises to 38° C. During this stage there is a certain loss of volatile carbonyl compounds, notably acetylene, from the crust.

In the second stage (the middle 50% of the baking time), the crust temperature rises to 130–140° C and intensive melanoidine formation occurs, accompanied by a build-up of carbonyl compounds in the crust as a result of melanoidine reactions. In particular, the furfural content of the crust, especially the top crust, increases, and the loaf assumes the characteristic coloration of well-baked bread. At the same time, the content of amino acids (aspartic and glutamic acids, threonine, tryptophan, methionine, valine and leucine) decreases throughout the whole loaf, the decrease being most marked in the crust, due to the consumption of these amino acids in the processes. During the second stage, the rate of reaction depends essentially on the crust temperature.

The third period (the last 25% of baking time) is characterised by an intensive build-up in the crust of compounds with bisulphite bonds, acetaldehyde and furfural. During this period, the temperature may fall slightly, and the chemical reactions, although similar to those of the second stage, now take place at constant rate.

used to synthesise ATP. When oxygen, and not light, is available the oxygen is used instead of the light to pump protons and so create the necessary potential.

An experimental observation on the anaerobic cells which is not so readily explained is a repeatable fast rise in pH when the light is first turned on. The rise is followed by a larger, slower fall leading to the net drop in pH already described. Oosterhelt and Stoerkenius suggest an initial influx of protons already forming a gradient across the cell membrane before the light is turned on. Adding an uncoupler causes the pH to rise, and the light effect is said to be abolished.

These results, although interesting in themselves, may also stimulate work on the visual pigments the function of which, beyond absorbing light, is still uncertain.

INSECTICIDES

Toxic to Cuticle

from a Correspondent

CHEMICALS have long been used in the control of insects of agricultural, veterinary and public health importance, but as their use has increased so resistance to the compounds has built up to the extent where some chemical pesticides now no longer provide economic and safe control. The use of other pesticides, notably some organochlorine compounds, has been severely restricted on account of their propensity for accumulation in food chains and consequently the pollution of the environment. A new class of insecticidal agents has recently been encountered which, apart from exhibiting a novel mode of action, also promises to be relatively innocuous as far as damage to the environment is concerned.

The laboratory evaluation of two members of this class of compounds 1-(4-chlorophenyl)-3-(2,6-dichlorobenzoyl) urea (designated PH 60-38) and 1-(4-chlorophenyl)-3-(2,6-difluorobenzoyl) urea (designated PH 60-40) is presented by Mulder and Gijswijt in a recent issue of *Pesticide Science* (4, 737; 1973). The toxicity of the two compounds has been evaluated in mice, rats and rabbits, where oral, intraperitoneal and dermal administration produces no toxic symptoms, and lethal doses are extremely high.

Several orders of insects were represented in the studies; lepidopterous and dipterous species proved the most susceptible. Insecticidal activity is manifested in the failure of larvae to moult or pupate so that the insect ultimately dies. PH 60-38 and PH 60-40 have so far shown no action on either the fecundity or on the viability of the eggs or larvae of treated adults. Toxicity to

larvae is evident only when taken orally. If the compounds are applied topically to the larvae, or applied to the nutrient solution of the food plant (some chemical insecticides are absorbed, transported and distributed throughout the plant, so that much of it becomes toxic to its insect predators), no toxic effects are elicited in the larvae.

Failure of the larvae to moult or pupate adequately is due to the disturbance in deposition of endocuticle caused by treatment. If the compounds are applied more than 24 h before a moult, no new endocuticle is laid down; and if treated shortly after a moult, strengthening of the endocuticle is prevented. Larvae treated before pupation either do not survive pupation, or emerge as abnormal adults. Treatment therefore results in the formation of a delicate cuticle which is unable to resist internal pressures, and larvae fail to extricate themselves completely from the exuviae at ecdysis. Death largely becomes apparent through loss of moisture, blackening, immobility, and strongly degenerated adipose tissue. So the compounds can be expected to be relatively benign and relatively specific in the right situation.

Development of biological means for controlling insects is providing more selective and non-persistent forms of control; but control of insects by biological means alone is unlikely to suffice in all instances. It is in integrated control programmes, where the blending of biological and chemical means of control ensures the use of the best features of each component, that the newly discovered action of this series of compounds could prove invaluable.

The compounds will be relatively selective in that they will act only on those insects actually eating the outer surfaces of plants. One drawback is that good coverage of plants with the pesticides must be achieved and is important for ensuring the effectiveness of compounds which act only after ingestion by pests. One other drawback is that treated larvae will continue to survive and destruct normally until the next moult, so that a certain amount of plant damage is inevitable. The efficiency of the insecticides will obviously depend on treatment of early instar stages.

ANIMAL PHYSIOLOGY

Bird Bodily Functions

from a Correspondent

THE study of avian physiology as a subject in its own right has blossomed in recent years and many well known enigmas have been solved. Progress has been made because proper physiological studies are being carried out, as speakers at a Zoological Society of London symposium on "Advances in

VIRUSES

Pox on the Elephant



THIS extraordinary photograph has been taken by Drs S. S. Kalter, R. J. Helmke and R. L. Heberling of the Southwest Foundation for Research and Education, Texas.

They write: The list of poxviruses grows larger each year with the isolation of serologically distinct entities, all with the biological characteristics of the poxvirus group, from the multitude of animal species. Andrews and Pereira (*Viruses of Vertebrates*, third ed., Williams and Wilkins, Baltimore, 1973) as well as Wildy (*Classification and Nomenclature of Viruses*, Monographs in Virology, 5, Karger, Basle, 1971) provide detailed information on the structure and other properties of the poxviruses. Electron micrographs depict the poxviruses as "brick-shape or ovoid complex particles" from such animal species as rhinoceros, buffalo, camel and so on. We have recently observed a configuration that makes us suspect that the elephant may also harbour a poxvirus.

Avian Physiology" (November 22-23) made clear. Thus the bodily functions of birds are no longer simply the subject of brief excursions by mammalian physiologists in search of 'models', zoologists seeking comparative information or poultry scientists requiring physiological background to husbandry problems. As several speakers implied, however, some workers on non-mammalian vertebrates still do not attempt to attain the high standards set by mammalian physiologists. A point made by M. J. Purves (University of Bristol), who dealt with cardiovascular control, was that the most valuable information is usually the most difficult to obtain.

There have been no more spectacular advances made than in avian respiration; these were dealt with by K. Schmidt-Nielsen who, with colleagues at Duke University, North Carolina, and one ostrich, was largely responsible for discovering how the respiratory mechanism of birds, which is completely

different from that of mammals, operates. V. A. Tucker, also from Duke, explained the energetics of bird flight—a field in which a knowledge of aerodynamics and physiology combined with excellent experimentation involving trained birds in a wind-tunnel has led to a real understanding. E. Skadhauge (University of Copenhagen) who was not only responsible for demonstrating unequivocally that salts and water from urine can be reabsorbed in the cloaca and rectum, but also for describing the transport mechanisms involved, has profitably extended his work to study the physiological adaptations of this process in relation to habitat.

Much of the work presented was of fundamental importance to an understanding of animal organisation, for example, M. Peaker (ARC Institute of Animal Physiology, Babraham) presented evidence that the osmoreceptors which control the salt glands in marine birds are located in or near the heart, and an obvious question is whether similar receptors monitor salt and water balance in other vertebrates. A field in which avian studies have been of great fundamental value and ahead of mammalian studies is the photoperiodic control of reproduction. Therefore the control of reproductive cycles and reproductive behaviour occupied a large part of the symposium. B. K. Follett (University College of North Wales) described experiments done in collaboration with D. S. Farner (who led the final discussion) and colleagues at the University of Washington, Seattle, to test the Bünning hypothesis of biological time measurement; such a test would not have been possible had it not been for the development of a sensitive radioimmunoassay for avian luteinising hormone. Environmental factors were not overlooked and R. K. Murton (Monks Wood Experimental Station) explained the way in which ecological adaptation will have to be explained in terms of control mechanisms. But natural environmental factors were not the only type considered and A. S. Cooke, also from Monks Wood, produced clear evidence that some pesticides do have a serious effect on an important reproductive process—formation of the egg shell.

Malcolm Peaker, who organised the symposium for the Zoological Society, explained that the programme had been arranged to cover avian physiology in its widest sense and to bring together those working on domesticated birds, as a part of poultry research, and those working on wild species. Thus this probably unique occasion for British ornithological science attracted a large audience with diverse basic interests, from field ornithology to neurophysiology and aeronautics to animal behaviour.

DRUG RESISTANCE

Bacterial Ingenuity

THE resistance to penicillin shown by many bacteria is in general mediated by a variety of penicillinases (β lactamases) which break the lactam ring of penicillin rendering it inactive. Semi-synthetic penicillins, such as methicillin and carbenicillin, have altered side chains making them far less sensitive to penicillinase action. The bacteria, as usual, have proved equal to the challenge.

In 1961, strains of *Staphylococcus aureus* resistant to methicillin appeared (Jevons, *Br. med. J.*, 1, 124; 1961) only a year after introduction of the drug. On investigation, this resistance did not seem to be mediated by the appropriate penicillinase (methicillinase) (Seligman, *Nature*, 209, 994; 1966).

A component of the resistance to carbenicillin and other penicillins shown by another important wound pathogen, *Pseudomonas aeruginosa* carrying the R factor RP1 (Lowbury *et al.*, *Lancet*, ii, 448; 1969), has now also been conclusively demonstrated not to involve a β lactamase (Curtis *et al.*, *J. gen. Microbiol.*, 79, 163; 1973). The β lactamase (IIIa) specified by this R factor hydrolyses carbenicillin relatively poorly, hardly sufficiently to account for the more than 1,000 times greater resistance shown by these strains. The Bristol group of Curtis *et al.* have now produced a mutant R factor which no

longer codes for the β lactamase but still confers appreciable resistance to carbenicillin and the other penicillins. Resistance to the other antibiotics specified by the R factor remained unchanged.

The mutant plasmid could not be reverted to β lactamase production by further nitrosoguanidine treatment and no spontaneous mutants have been obtained, so it is probable that the β lactamase genes have been deleted. This relatively high level of residual resistance is not due to a chromosomal gene as it disappears when the cells are cured of the plasmid by dodecyl sulphate treatment, neither is it due to a change in the levels of penicillinase Ia production by the plasmid-less cell.

The mutant R factor has been transferred to *Escherichia coli* K12 in which it also expresses a similar resistance pattern.

Penicillin resistance mediated by penicillinases was relatively straightforward to overcome by building new semi-synthetic penicillins, but even when the basis of this new type of resistance is uncovered it may prove difficult to circumvent, as it is much more likely to be due to structural and permeability changes in the cell itself. Methicillin-resistant *Staphylococcus aureus* and carbenicillin-resistant *Pseudomonas aeruginosa* now represent a significant problem in the management of infected wounds.

Pole Tide in the North Sea-Baltic Area

AS Lord Kelvin pointed out in 1876, a wobble of the Earth will induce an oceanic tide. In the case of the Chandler wobble, now known to have a period of about 14 months, the induced tide (pole tide) has a calculated equilibrium amplitude of only about 0.5 cm, but is important because, if really equilibrium in form, it will lengthen the period of the wobble by about a month compared with the period for an Earth without oceans. Moreover, the existence of such a pole tide raises the possibility that the oceans may be a primary sink for the wobble energy; and in so far as excitation and decay are related problems, any information on modes of dissipation could throw light on the vexed question of the excitation mechanism of the Chandler wobble.

Measurement of the pole tide is difficult because of the high background noise in sea level, and so reported determinations are few. The most successful result was obtained many years ago by Haubrich and Munk (*J. geophys. Res.*, 64, 2373; 1959), who found a fairly prominent peak in the sea level spectrum at about 0.84 cycles a year, corresponding to a period of 1.19 yr (that is, about 14 months). Since then, however, the

problem has attracted little attention except that data published by Maksimov and Karklin (*Dokl. Acad. Nauk SSSR*, 161, 9; 1965) suggest a larger than equilibrium pole tide in the Baltic.

In *Nature Physical Science* next Monday (December 17), Miller and Wunsch report a further attempt to measure the pole tide, this time based on all available sea level records of more than 25 yr duration published by the Association d'Océanographie Physique. Most of the examined records were ambiguous; in most cases the pole tide did not rise above the background, and where it did it was weak. For records from the North Sea-Baltic area, however, the search was more successful, the pole tide being clearly visible at a frequency of 0.84 cycles a year. Moreover, Miller and Wunsch have also been able to show that there are systematic variations in the amplitude of the tide, from 0.7 cm at the southern end of the Netherlands coast to 1.8 cm at the northern end. In general, amplitudes rise markedly going eastwards from the North Sea into the Baltic and the Gulf of Bothnia, although the values in the constricted waters of the Kattegat are much lower than the general trend.

METEOROLOGY

Solar Activity and the Weather

from a Correspondent

A SYMPOSIUM on "Possible Relationships Between Solar Activity and Meteorological Phenomena" was held at the Goddard Space Flight Center (GSFC) on November 7-8, sponsored by NASA, in cooperation with the University Corporation for Atmospheric Research (UCAR) and the American Meteorological Society. In his welcome and opening remarks, J. C. Fletcher (NASA) dedicated the symposium to C. G. Abbot for his pre-eminent pioneering work in the measurement of the solar constant and the search for solar-meteorological relationships. Dr Abbot, who in 1972 celebrated his one hundredth birthday, was present in the audience.

In the two opening papers W. O. Roberts (UCAR) and J. Wilcox (Stanford University) reviewed some of the statistical evidence to suggest a meteorological and climatic response to variations in solar activity. The statistical evidence presented includes:

- (1) The recurrence of droughts in the high plains of the United States related to the double sunspot cycle of 22 yr.
- (2) An increase in meridional circulations and blocking patterns at high and intermediate latitudes, especially in winter, generally related to high solar activity.
- (3) Glacier advances and other indicators of cold climate in late glacial and post-glacial time occurred during intervals of weak solar activity, and a similar percentage of glacier recessions and warm climate indicators occurred with high solar activity.
- (4) In some regions of the globe the atmospheric pressure increases after geomagnetic storms, whereas in other places the pressure decreases. The reaction time is about 3 d and tends to increase with decreasing latitude.
- (5) High latitudes show higher average pressures at sunspot maximum than at minimum and at high solar activity there is a mass displacement of air toward high latitudes.
- (6) Developing 300 mbar low pressure trough systems entering or forming in the Gulf of Alaska 2 to 4 d after a sharp rise in geomagnetic activity tend to be larger than average size, and the vorticity area index of these trough systems reaches a maximum about 1 d after the passing of a solar magnetic sector boundary, followed by an increase of magnitude approximately 10% during the next 2 or 3 d.
- (7) The mean change in height of atmospheric constant pressure levels during the first 24 h after a flare is greater than

may be expected from mere random fluctuations in height.

(8) Significant increases in westerly winds at the 700 mbar level in the longitude belt from 90° W to 180° occur ~3 or 4 d after magnetic storms.

(9) Thunderstorm activity has been correlated with the Earth's position in a solar magnetic sector and solar hydrogen- α flare activity.

A. J. Dessler (Rice University) discussed some problems in coupling solar activity to meteorological responses since most geophysical phenomena have a high intrinsic noise level and there is no acceptable mechanism to help organise the data into a manageable search. The energy provided by meteorological phenomena is (essentially) entirely provided by sunlight. The solar constant is about 10^8 erg cm⁻² s⁻¹. The available energy flux to the atmosphere of the solar wind and the interplanetary magnetic field for undisturbed conditions is less than one millionth of that of the solar constant, and the meteorological response, related to solar activity energy variations, of less than 0.1 to at most 10^3 erg cm⁻² s⁻¹. So a 'trigger mechanism' is envisioned since brute force will not work. Dessler discussed the hypothesis of C. O. Hines (University of Toronto) whereby magnetospheric convective motions, which are intensified during magnetic storms, change the vorticity of the lower atmosphere at or near auroral latitudes by viscous coupling. Dessler concluded that there seems to be enough power within the magnetosphere to cause such changes in vorticity, if the power can be directed and coupled effectively.

Another mechanism discussed by Dessler is heating accomplished by direct particle bombardment in the auroral zone. An outstanding intense auroral beam has an energy flux less than 10^{-3} that of sunlight. The heat capacity of the upper atmosphere is so small that the effect of absorbing this energy flux is profound. But the upper atmosphere is thermally isolated from the lower atmosphere by two temperature minima—at the tropopause (15 km) and at the mesopause (80 km). Energy may be converted to forms that can penetrate through these temperature minima to the troposphere, such as infrared radiation and infrasonic waves. But with a power input of only 10^{-3} that of sunlight, Dessler believes it is hard to imagine that the small fraction of this energy that would go into either component would provide a sensible perturbation to the tropospheric system. Roberts believes that a promising trigger mechanism involves the modification of the atmospheric radiation budget

through the sudden formation of cirrus clouds at the tropopause following solar activity.

R. C. J. Somerville reported on numerical experiments on short-term meteorological effects of solar variability performed at the NASA Goddard Institute for Space Studies directed by R. Jastrow. The GISS group carried out a set of numerical experiments to test the short-range sensitivity of a large atmospheric general circulation model to changes in solar constant and ozone amount. On the basis of the results of 12-d integrations with very large variations in these parameters, it was concluded that realistic variations would produce insignificant meteorological effects. The GISS study also concluded that any causal relationships between solar variability and weather, for time scales of 2 weeks or less, will have to rely on changes in parameters other than solar constant or ozone amounts, or upon mechanisms not yet incorporated in the model.

W. W. Kellogg (National Center for Atmospheric Research, Boulder) discussed needed measurements and observations including:

- Continuous monitoring of the energy and pitch angle distribution of geomagnetically trapped electrons and protons in order to determine when they are precipitated into the lower ionosphere.
- Monitoring from balloons in the region of the tropopause the incidence of ionising radiation and any accompanying changes of temperature, conductivity, ozone amount or ultraviolet flux.
- Continuous monitoring from a satellite of absolute solar flux in the near ultraviolet, between 2,000 and 3,000 Å.
- Monitoring global ozone distribution in the region above 30 km.
- Observations of wind systems in the mesosphere and lower thermosphere.
- Detection of high latitude cirrus cloud formation through optical techniques or through the effect of a cirrus deck on the upward infrared radiation in the atmospheric window.
- Satellite optical and radio monitoring of global thunderstorm activity.

In his symposium summary S. I. Rasool (NASA) cautioned against the current practice of correlating solar variations with localised, isolated weather effects instead of global responses. He stressed the importance of the stratosphere as a buffer for solar-meteorological responses and commented that due to its tremendous inertia the relaxation time of the troposphere is so large that short-term tropospheric responses to solar variations cannot be easily identified.

MISCELLANY

Christmas Prize Quiz

Answers to the following questions are all to be found in the pages of *Nature* this year. A prize of a year's subscription to *Nature* is offered for the first correct set of answers opened by the Editor after January 31, or for the best entry.

1. At the start of the year, the Science Research Council became committed to spending £3 million a year on a European venture. What is the venture and who are Britain's partners in it?

2. David's departure caused a New Year surprise. Where did he depart from?

3. From the opposite end of the political spectrum, an arrival in London also seemed surprising in the light of previous refusals. But the pleasure of the scientist concerned was short lived. Who was he and what happened to him in August?

4. After recent moves in musical chairs of astronomy, who is now the Director of the Institute of Astronomy in Cambridge? Who was his predecessor and where is he now?

5. Horns, cols and a hanging valley found by a mariner surprised astronomers. What does their presence indicate?

6. The Council for Scientific Policy died and was reborn in 1973. What is the new name of the Phoenix and who has been chairman of it?

7. Where do we receive communication of the form CCCDDCCCCDR?

8. Which countries joined the post-Apollo programme at the last minute?

9. Dr Dixie Lee Ray struck a blow for women's liberation in 1973. How?

10. In the autumn a minister apologised for misleading the House. Who was he and what was the subject under discussion?

11. Who said "we shall be spending thousands of pounds educating young people in social sciences and I cannot imagine any more foolish waste of money"?

12. When is a panda not a raccoon?

13. Which Nobel Prizewinner accused President Nixon of lunatic behaviour? In what context?

14. Nina is approaching retirement. How old will she be when she goes?

15. Who is the latest head of Britain's nuclear power industry?

16. *S. muticum* was found attached to a sheltered shore in Hampshire. Where did it originate?

17. Thorarinsson, Steinthórsson, Einarsson, Kristmannsdóttir and Oskarsson reported on what violent activity?

18. Laithwaite, Ford and Elliot had

their spring hopes dashed. How?

19. William D. Ruckelshaus granted a temporary reprieve in April. Who to?

20. OH471 extended our knowledge of the Universe. How far?

21. It was suggested in January that there may be 35 million plastic bottles floating in the Pacific. How many red rubber sandals correspond to this?

22. Which Soviet scientist was elected to the National Academy of Sciences in May?

23. A pattern of climatic change affecting the whole Northern Hemisphere was pointed out this year. If the pattern is more than a coincidence, when will the Sahara stop moving south?

24. In 1973 there was a change of editorship of *Nature*. This has happened only four times in 104 years. How many Editors have there been and how many are still living?

25. The power of Flowers gave way to that of another in 1973. Who?

26. What do birds come from?

27. Where were a Jumbo number of members faced with a 100% increase in fees?

28. What do baboons use for "napkins"?

29. France's governmental loss was America's academic gain. Who left what to go where?

30. What new society did Paul Sieghart found?

31. What are thrusting everywhere?

32. Skylab followed Apollo. How has Heseltine helped with Skylab's heir?

33. CREST and SPIT; who, what or where?

34. What is IUPAP and how did it fail to meet Professor A. V. Voronel?

35. The Think Tank leaked in September. Where did the leak occur?

36. How old *Homo*?

37. How did *Dimetrodon grandis* keep his cool?

38. A matrimonial binary system did not, after all, make the transition from California to Britain successfully. What was the result?

39. Jesus gained a new master. Who?

40. BASS was conceived by BA. When is the birth?

Entries to: QUIZ, *Nature*, 4 Little Essex Street, London WC2R 3LF.

What's in a Name?

AMONG the many botanists in the British Isles are A. M. M. Berrie (Glasgow), K. D. Gardiner (horticulturist, Dublin), B. E. Juniper (Oxford), R. P. Moss (Birmingham), B. L. Moss (Newcastle upon Tyne) and F. Rose (King's College, London).

Fish biologists and oceanographers have in their ranks J. C. Chubb (Liverpool), H. Fish (Essex River Authority), G. R. Fish (Fish Research Division, Rotorua), G. C. Trout (Fisheries Laboratory, Lowestoft) and P. J. Herring (Institute of Oceanographic Sciences).

Zoologists include M. W. Fox (Washington, author of *Behaviour of Wolves, Dogs and Related Canids*), ethologists and collaborators, Robin Fox and Lionel Tiger. It has also been brought to our notice that two zoologists from Birmingham, Drs Cock and Cohen, once collaborated on a paper on the combs of hens.

Miscellaneous: G. C. Cheeseman (National Institute for Research in Dairying, Reading), J. W. G. Musty (Inspectorate of Ancient Monuments, DOE), P. M. Chalk (soil scientist and expert on limestones, Wisconsin), D. L.

Coffin (expert on air pollution and environmental health), R. S. Pore (microbiologist, West Virginia), R. T. Severn (civil engineer and consultant for the Severn Barrage, Bristol), E. H. Shove (engineer, City University) and T. E. Gambling (Department of Accounting, Birmingham).

No comment: Z. Ovary (contributor to a conference on obstetrics and gynaecology), Miss Mercedes B. Concepcion (Philippines representative at the Second Asian Population Conference) and Chuck Despina (producer of the BBC TV Horizon programme on acupuncture).

We beg the forgiveness of those readers who feel that their names should have been included but are not; to those who feel that their names should not have been included but are, we offer the reminder that the chief compiler of this information over the past few years has been Sarah Bunney, our News and Views Editor, whose own name has more than one connection with the title of this journal. The fact has not, it seems, escaped the attention of certain telephone callers.

Solution to the BA Cartoon

We asked for identification of the people depicted on the front cover of *Nature*, August 17, 1973. The prize-winning solution given here came from D. P. L. Green, Department of Biophysics, University College, London.

My solution is, proceeding in a clockwise direction from bottom centre: (1) Sir Roderick Impey Murchison (1792–1871); (2) Charles Babbage (1792–1871); (3) Thomas Henry Huxley (1825–95); (4) Dr Richard Owen (1804–92); (5) John Tyndall (1820–93); (6) Sir John Frederick William Herschel (1792–1871); (7) Sir Charles Lyell (1797–1875); (8) George Julius Poulett Scrope (1797–1876).

The ready identity of three of the eight figures immediately raises several interesting questions.

(1) Do the characters depicted bear some close relationship to the structure of the British Association (BA) in 1865?

(2) What relationship do the characters bear to their immediate surroundings?

(3) How much did the artist know and understand of the immediate scientific background of each character?

(4) To what extent do the characters bear a resemblance to other known portraits?

In the Report of the BA for 1865, Sir Roderick Murchison is listed as one of the three trustees as well as the President of Section C, Geology, and Vice-President of Section E, Geography and Ethnology. Dr Richard Owen, as he then was, is listed as an ex-officio member of council, having been President of the BA for the meeting at Leeds in 1858. T. H. Huxley is listed only as a member of the BA. This indicates that we need not be looking for a well defined relationship between the figures and the structure of the BA in that year.

It seems probable that the average reader of *Punch* would not know the appearance of many distinguished men of science of the day. It seems equally probable that the cartoon would lose much of its interest if the reader failed to recognise readily the men depicted.

I turn now to the second and third questions. It is necessary to recount briefly a few biographical details of both Owen and Huxley immediately prior to 1865. Owen was well known in London for having delivered a course of public lectures for 1859, 1860 and 1861 at the Museum of Practical Geology in Jermyn Street. His subject was the fossil remains of animals. His views

were summarised in the Rede lecture delivered at Cambridge in 1859 under the title of "Classification of Mammalia". In April 1860 he wrote an attack on Darwin's *Origin of Species* for the *Edinburgh Review* and Darwin believed him to have inspired the Bishop of Oxford's hostile notice of the same book in the *Quarterly Review*. As for T. H. Huxley, the greater part of his published work for 1855–59 was on fossil forms. The most important problem which then presented itself, both in contemporaneous and fossil form, was the composition of the skull. The doctrine prevalent in England at the time was that which Owen had learned from Goethe and Oken. This method of description obscured the actual anatomical relations of the parts described. Huxley, in his Croonian lecture of 1858 entitled "On the Theory of the Vertebrate Skull", gave a statement of cranial structure which differed from that of Owen, but which Owen failed conspicuously to refute. It is this controversy, rather than their differences over the *Origin of Species*, which I take the audience of curious animals to refer to. Most conspicuous, of course, are their skull-like heads. It is probable that both controversies explain their polite wrestling.

This shows in some detail that the characters do bear some relationship to their immediate surroundings, and this is intuitively obvious, I think, from the general nature of the cartoon. It also shows that the cartoonist had some knowledge of the scientific background of each character.

It now remains to attempt a brief answer to the fourth question, and I will start with Sir Roderick Murchison. In *The History of the Geological Society of London* (London, 1907) by H. B. Woodward there is a reproduction of a painting by Pickersgill of Murchison. From this, it can be seen that the cartoon is not a good likeness. A better likeness exists between the cartoon and the reproduction of an old engraving of Murchison in *The First Hundred Years of the Geological Survey of Great Britain* (HMSO, 1937) by Sir J. Smith Flett. Similarly, a comparison of the depiction of Huxley by the cartoonist with the many extant pictures of Huxley

from this period again serves to underline that we can only expect rather poor likenesses of the figures involved. There is one other problem which should, perhaps, be mentioned at this stage because it is of some importance later. It is not clear how the cartoonist derived his knowledge of the appearance of the figures. This is particularly important if it were to be derived from engravings, because these naturally have the image reversed. This leads to a key feature of identification, the parting of the hair, to be subject to some doubt.

I shall now discuss each separately.

Sir Charles Lyell

The two figures at bottom right are clearly geologists. The most distinguished geologist of his age was undoubtedly Sir Charles Lyell. A photograph in the possession of the Geological Society, which is reproduced in the history of the society mentioned above, places this beyond doubt. Note the pronounced jaw which he possessed, and which comes out fairly clearly in the cartoon.

George Julius Poulett Scrope

Scrope is an exact contemporary of Lyell. In 1828, Scrope published *Considerations on Volcanos* and this enunciated emphatically the doctrine subsequently developed by Lyell and called by him Uniformitarianism. To this extent he was a forerunner of Lyell and their views were generally very similar. He sat as the Member for Stroud from 1833–68, and although he spoke seldom, if at all, he conducted a vigorous campaign of pamphleteering on most social issues of the day, upon which he held what were then considered to be advanced views. As a result, his work as a geologist had slackened considerably. Nevertheless, Scrope and Lyell were considered by their contemporaries as joint protagonists, and the cartoonist's depiction of the two figures together is evidence of the association. (The only other pair is that of Huxley and Owen, and, as already mentioned, they were also associated in the public mind together, although for different reasons.) Finally, there is a photograph of Scrope in the possession of the Geological Society which is reproduced in



The *Punch* cartoon reproduced on the cover of the August 17 issue of *Nature*. It was first published in September 1865. (Reproduced by permission of *Punch*.)

the history mentioned above and which bears a good likeness. The only serious alternative candidate, on the criterion of appearance, is Sir Andrew Crombie Ramsay (1814–91), President of the Geological Society from 1862–64. The objections to this identification rest on the fact that, despite his age in 1865, he was only beginning to rise to public notice and he was not at all associated, in the same manner as Scrope, with Lyell's earlier career.

Sir John Frederick William Herschel

I present the evidence for this ascription in a different order from that for the two above. There exists a photograph of Herschel, taken in 1867, when he was seventy-five. This is reproduced in *The English Face* (London, 1957) by David Piper. Anyone who has seen this picture will find it very difficult to believe that the figure can be anyone else. Nevertheless, there are some problems caused by the surroundings. In 1865 Herschel had been in the forefront of scientific life for fifty years. In 1830 he stood as the 'scientific' candidate for the Presidency of the Royal Society against the Duke of Sussex, and was narrowly defeated. In 1839 he refused a proposal to succeed the Duke. He wrote on a wide variety of subjects. In a paper "On the Chemical Action of the Rays of the Solar System on Preparations of Silver and other Substances" he first described the use of sodium hyposulphite as a fixing agent and used the terms positive and negative in their photographic senses also for the first time. Earlier, he had improved the objectives of microscopes in addition to other important work in optics. He

was always interested in technological improvements, and was one of the jury for scientific instruments at the Great Exhibition. From 1850–1855 he was Master of the Mint and in 1865 he was a member of the committee of the BA which organised the use of balloons for meteorological and physical observations.

This outline constitutes only a small part of his activities but it does serve to explain most of the surroundings in the cartoon. The peculiar device which the figure is holding is less clear. It may be a reference to his photographic work or to his work on microscopes. In either case, the legs of a small man sticking out are, to me, obscure. As for the steam hammer, this again presents difficulties. It could, of course, refer to some process used either at the Mint or in the manufacture of instruments. There is another explanation. One other possible candidate for the figure is the inventor of the steam hammer, James Nasmyth (1808–90). Nasmyth was himself an instrument maker and keen astronomer, and he knew Herschel well. On Herschel's death, Nasmyth put him "supremely at the head" of all the scientific men of his acquaintance. Consideration of Nasmyth as the figure depicted is not itself without objections, however. There is no connection to my knowledge between him and balloons and I do not think that he was as conspicuous a public figure as Herschel. He was also much younger, yet the figure under discussion is the oldest in appearance in the cartoon. Finally, although Nasmyth was melancholic in appearance, rather as Herschel, he also possessed a pronounced curl of hair in the centre of his forehead.

John Tyndall

The surroundings of this figure include the series of bottles with signs upon them. These signs are Berzelius's notation for certain metallic elements and are derived from the astrological notation for certain members of the Solar System. It marks this figure out as a physical scientist. The hot poker in the left hand, I take it, indicates a connection with heat.

In 1853, Tyndall became Professor of Natural Philosophy at the Royal Institution where he was Faraday's closest colleague until the latter's death, whereupon Tyndall succeeded him as Director. His early researches were on magnetism, but by 1865 he had for many years been interested in heat. His book *Heat Considered as a Mode of Motion* was published in 1863. It was widely read. It is difficult not to think that the character jumping in response to the hot iron is not a visual pun on heat as a mode of motion. There is one problem with this ascription, the parting of the hair. Tyndall parted his hair on the left. In mitigation may be stated two facts. First, his hairline receded markedly on each side of his head and this may be loosely represented by the cartoonist as a parting. Second, there existed in about 1860 an engraving by H. Adlard of Tyndall, with Faraday, in which, of course, the parting is reversed. This problem has been discussed in the preamble. (I would just add that a cartoon of T. H. Huxley, reproduced in *T. H. Huxley* (London, 1959) by Cyril Bibby shows Huxley with his parting on the wrong side.) The overall likeness of the cartoon to Tyndall is tolerable, especially in respect of the beard.

Charles Babbage

This is again not without difficulties in ascription. The three salient features of the cartoon are the numbers, the fish and the price tag. The numbers I take to refer either to an arithmetician or to somebody whose interest lay in computation. The figure who immediately springs to mind in both cases is Charles Babbage. He wrote widely. His *Reflections on the Decline of Science in England and Some of its Causes*, published in 1830, was an attack on the management of the Royal Society which contributed materially to the founding of the BA in 1831 at York. In 1832 we have the *Economy of Machines and Manufactures*; in 1848, the *Thoughts on the Principles of Taxation, with reference to a Property Tax and its Exceptions*, and in 1851, *The Exposition of 1851; or, Views of the Industry, the Science, and the Government of England*. In addition he wrote papers on a great range of subjects, but he is, of course, best remembered nowadays for his work on calculating

machines. In the introduction to the second edition of *The Exposition of 1851* Babbage added, "On one or two points I differed entirely from the opinion of those to whom its management was confided. The questions of the site of the building and of affixing prices to articles exhibited were the most important."

Chapter VIII is wholly devoted to prices and a description of how Babbage saw a free market operating. On page 93 we have, as one of several examples, the following:

"Different prices for the same article are often demanded by retail tradesmen according to the supposed position of the purchaser. Fish, for example, which varies much in price and is at times very cheap will seldom be found charged to the household bill much below the average price. . . ." Babbage was widely known to the public for his forthright views, and although this was written fourteen years before 1865, it is just the sort of thing which would become stuck in the public's mind.

If it is Babbage, the figure was the same age as Murchison and Herschel and yet, of the three, is clearly the youngest. In a photograph of Babbage, reproduced in *Irascible Genius; A Life of Charles Babbage* (London, 1964) by Mabothe Moseley, he looks younger than he was. This picture also shows the texture of his hair and the marked indentation at the parting shown in the cartoon. Finally, the figure in the cartoon looks somewhat impatient and irascible.

Let me be the first to state that I am not wholly convinced of my own solutions. It is in the nature of the problem that I could not arrive satisfactorily at the solution merely by elimination of alternative candidates because of the general paucity of my knowledge. Rather, I have tried to make sensible guesses and justify them to the best of my means.

Note

We also said that a mention of the Stodare trick appeared beneath the cartoon. For good measure Dr Green appends the following information:

The Stodare trick mentioned in the caption is one of two named after Colonel Stodare, the stage name of John English (1831-66). In the *Encyclopaedia Britannica* for 1910 there is a reference to a trick as follows:

"The sword and basket trick was common in India many years ago. In one form it consisted in inverting an empty basket over a child upon the ground. After the child had secreted himself between the basket bottom and a belt concealed by a curtain painted to look like the actual wicker bottom, a sword was thrust through both sides of the basket, the child screaming and squeezing upon the sword a blood-coloured liquid from a sponge. When the performer upset the basket the child could not be seen; but another child, similarly costumed, suddenly appeared among the spectators, having been supported by a pair of stirrups under the cloak of a confederate among the bystanders."

Chess Problems

THE most familiar form of chess to most people is the serious and competitive game, the world of match play, tournaments and grand masters. There exists, however, a different form of chess, the quieter branch of problems and studies where the chief aim is generally that of amusement only.

One type of chess problem is to present a situation which appears on the surface to be quite easy to solve but which in practice does not turn out to be so simple. For example, a problem requiring a 'mate in one' probably gives the impression of being insultingly simple, yet examples of this type of problem do exist where the solution is not immediately obvious. Problems 1 and 2 below are both mates in one and may not delay most solvers long from their Christmas turkeys, but it is important to be quite sure that the sole correct solution has been found for each problem.

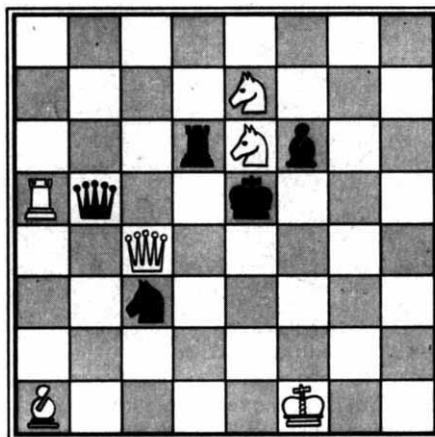
Problem 3 is based on an idea demonstrated by Sam Loyd over a hundred years ago. It is a mate in two, which may give certain alternating impressions during the course of solving it:

first, 'impossible'; second, 'trivial'; third, 'impossible' once again, before the real point of the problem is spotted.

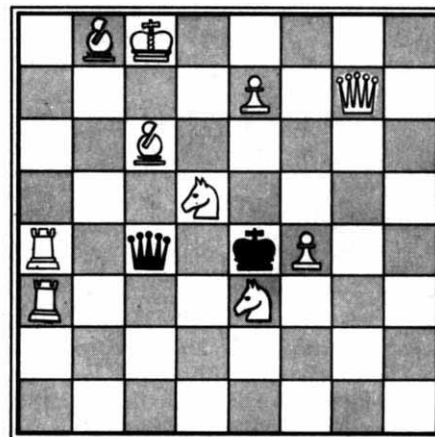
There is a tradition amongst problemists that a problem position must be a 'legal' one. In other words, it must be a position that could, theoretically, be reached from the starting position of a game of chess (unless it is specifically stated to be a "fairy chess" problem). This has given rise to some puzzles where moves have to be worked out in a retrograde fashion in order to determine that only a single sequence of moves could have been 'legally' played immediately leading up to the position in hand. Problem 4 is an adaptation of an old idea showing that only one 'legal' move is possible as White's immediately preceding move to the position in the diagram. But a modicum of retrograde analysis is necessary to demonstrate that the solution is a unique one, and it is worth noting that the line-up of Black pawns on the queen's rook file is a relevant feature of the problem.

J. P.

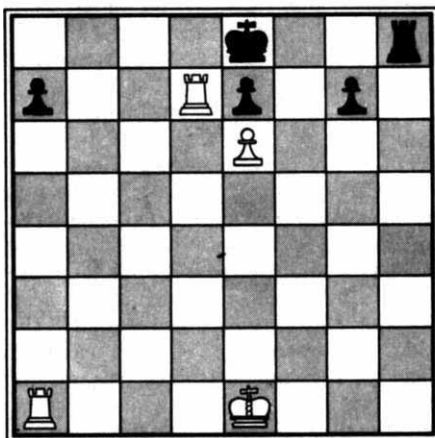
(Solutions will be found on page 438 of this issue.)



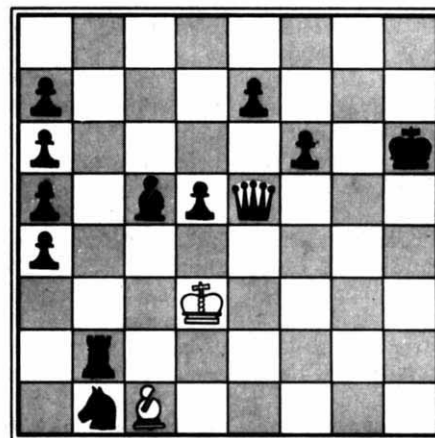
1 White mates in one move.



2 White mates in one move.



3 White mates in two moves.



4 What must White's last move have been?

Molecular Evolution of Myoglobin and the Fossil Record: a Phylogenetic Synthesis

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An ancestral myoglobin chain has been deduced by comparing the differences in the amino acid sequences of eighteen living species and assessing from the fossil evidence the probable times of divergence of their ancestors.

THE molecular evolution of myoglobin can be investigated by comparing the differences in the eighteen species for which its amino acid structure is known¹⁻¹⁷ (see Fig. 1 for the complete sequence of human myoglobin), and probable times of divergence of the ancestors of these species can be assessed from fossil evidence. The biochemical and palaeontological information has been integrated in a cladogram and a possible ancestral myoglobin chain deduced. The reconstructed pathways of mutation support most of the assumed phylogeny but also suggest possible modifications, and provide information about rates of molecular evolution.

The number of amino acid differences between species is shown in Fig. 2. Although similarities may result from close relationship, or convergence, or possibly transduction^{18,19}, we have not taken the latter into account.

The evolutionary study of myoglobin cannot be divorced from its function. Myoglobin resembles haemoglobin at the haem contacts, but not at those sites peculiar to haemoglobin which are responsible for haem-haem interaction^{8,20}. The internal hydrophobic residues are similar in all globins. Among the eighteen mammalian myoglobins mutations have been fixed at only sixty-seven sites out of 153. These involve sixty out of 120 surface residues (including three of ten external haem contacts) and only seven out of thirty-three internal sites (including two of twelve haem contacts). At all three external haem contact sites which have accepted a change, the residue is constant in seventeen out of eighteen species.

MAN	CHIMPANZEE	GIBBON	BABOON	MACAQUE	WOOLLY MONK	SQUIRREL MONK	MARMOSSET	GALAGO	SPORT LEMUR	HARBOR SEAL	PORPOISE	DOLPHIN	SHEEP WHALE	HORSE	OX	SHEEP	KANGAROO	LAMPREY	GLYCERA
1	1	6	7	18	17	14	23	22	24	20	26	25	19	25	25	22	105	102	MAN
2	7	8	17	18	15	24	21	23	19	25	24	18	28	24	23	105	102	CHIMPANZEE	
5	6	15	16	13	24	23	25	21	27	26	20	30	26	23	105	103	GIBBON		
1	13	12	11	22	21	23	22	27	26	18	29	25	22	106	103	BABOON			
12	12	10	21	21	23	22	27	26	18	29	25	22	106	104	MACAQUE				
4	4	21	19	25	25	31	29	19	29	25	22	107	105	WOOLLY MONK					
4	23	17	26	27	32	31	19	30	26	23	107	106	SQUIRREL MONK						
	23	17	23	24	29	27	17	28	24	22	106	105	MARMOSSET						
	21	28	25	30	27	22	30	27	30	102	105	GALAGO							
	17	18	23	24	12	23	20	24	102	107	SPORT LEMUR								
	17	23	26	20	26	27	104	105	HARBOR SEAL										
	12	15	16	23	23	28	105	103	PORPOISE										
	9	21	29	26	32	104	103	DOLPHIN											
	19	30	27	31	103	102	SHEEP WHALE												
	18	16	25	105	103	HORSE													
	6	35	102	104	OX														
	31	103	104	SHEEP															
	103	103	KANGAROO																
	97	LAMPREY																	
		GLYCERA																	

Fig. 2 Matrix showing the number of amino acid differences between the myoglobins of eighteen mammals and the haemoglobins of lamprey and *Glycera*.

Of the numerous differences between haemoglobin and myoglobin, four deserve comment. (1) There are thirteen residues participating in salt bridges which help to maintain the shape of the monomer^{21,22}. With one exception (E5 Lys) these residues differ between the globin families, but amongst myoglobins only five of the thirteen have accepted mutations, and these are highly conservative (Asp $\leftrightarrow$ Glu, Lys $\leftrightarrow$ Arg), except for kangaroo (G3 Gln instead of Lys). (2) In the myoglobins G5 Leu is invariant, and in the haemoglobins (except *Chironomus*) G5 is Phe (ref. 23), which helps to maintain the haem plate in the more upright position in deoxyhaemoglobin²⁴. An abnormal human haemoglobin "Heathrow" also has Leu at β G5 (ref. 25). This variant has a very high oxygen affinity, and it is probable that G5 Leu has a part in conferring that characteristic on myoglobin. (Myoglobin, being a monomer, would be expected to have a high oxygen affinity like a single haemoglobin chain. (3) H23 Tyr is helical in myoglobin but

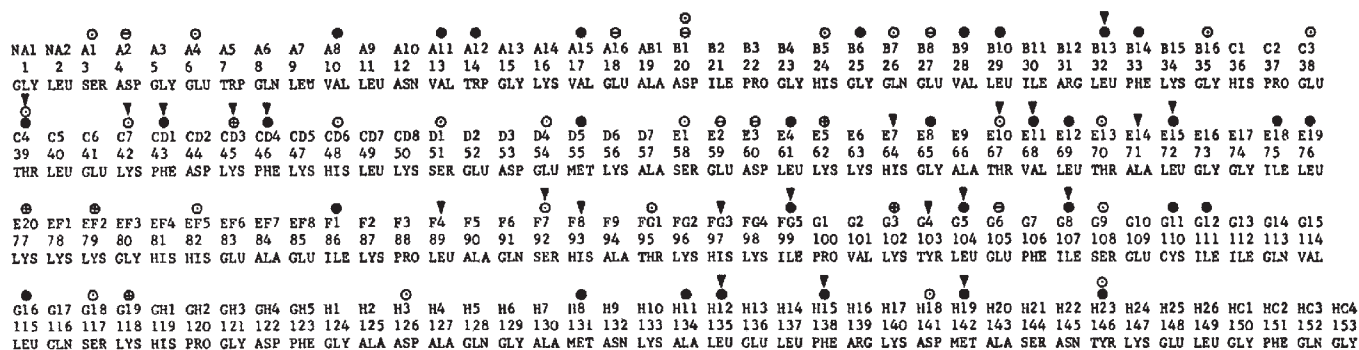


Fig. 1 Complete sequence of human myoglobin (with helical and sequential numbers). ▼, Haem contacts; ●, internal residues; ⊕, ⊖, charged residues forming salt bridges; ○, residues involved in hydrogen bonding.

non-helical (HC2) in haemoglobin²⁶ where this invariant Tyr participates in the transition from the deoxy- to oxy-conformation of the tetramer²⁷. In the monomeric myoglobin the hydrogen bond which this residue forms with the main chain at FG5 (refs 28 and 29) must be of singular structural importance. (4) In the myoglobins the C helix and the CD interhelical bend form an external flat plane where the charged residues C3, C6, CD2, CD5 and CD8 are invariant. The invariant FG4 also falls into this plane. Preservation of this region may be due to interaction with other molecules or mitochondria during the facilitation of oxygen diffusion^{30,31}. In haemoglobin, C3, C6 and FG4 are tetrameric contacts²⁹, and the strict preservation of a difference in this part of the myoglobin surface may guarantee its monomeric character.

Haemoglobins and myoglobins have a common ancestral polypeptide chain and so two distant haemoglobin chains have been compared with those of mammalian myoglobins (Fig. 2). One was the major monomeric component of haemoglobin from the marine worm *Glycera*³² and the other was haemoglobin V from the lamprey *Petromyzon*³³. These forms have evolved separately from the ancestral stock leading to mammals for not less than 540 and 420 m.y. respectively. In both cases 136 sites were comparable, insertions and deletions having been eliminated. In comparing *Glycera* with lamprey twenty-seven of 124 homologous sites were identical.

The similarity in the number of amino acid differences between these haemoglobins and the myoglobins is remarkable (Fig. 2). In each case they average 104.5 ± 2.5 residues, in spite of the fact that the mammalian myoglobins average twenty-five differences between them. But the apparent constant rate of molecular evolution when *Glycera* and lamprey globins are compared with those of mammals reflects amino acid differences without taking into account that during a minimum of 400–500 m.y. of divergence even the minimum mutation distance is inaccurate^{34,35}, because undetected mutations can lead to errors greater than 100%.

When two globin chains are compared ignoring insertions and deletions, then irrespective of their relative divergence (even considering plant haemoglobin)³³, a minimum proportion of approximately 16% of their residues is the same. Except for the invariant F8 His and CD1 Phe the location of these differs from pair to pair. There is a limit to the number of amino acid differences that a given molecule can accept and it can be argued that the variable sites are interdependent because a new side chain will affect its neighbours in the tertiary structure, and so put some constraint on the residues involved in the folding of the molecule. A similar observation has been made for cytochrome *c* (ref. 36). Setting aside convergence and transduction it appears that the characteristic tertiary structure of the globin family has been maintained within a small degree of variation during at least 540 m.y. of evolution³⁷.

Phylogenetic Chart

It is now essential to explain the basis of our phylogenetic chart (Fig. 3). The "Geological Society Phanerozoic time-scale (1964)"³⁸ has been used with modifications suggested in the Supplement³⁹. Necessary qualifications regarding Tertiary dates, especially for the Miocene, are fully discussed by Berggren⁴⁰. The pattern of branching largely reflects the grouping of the species in systematic classification but gives no indication of rank in systematic hierarchy. To determine the positions of branching we asked the question: at what point in time can it be shown that the ancestors of these two living forms had diverged? We rejected speculations regarding earlier dichotomies which require assumptions about fossils not yet found, and in consequence many of our points are likely to be later than the actual event.

Trichotomies do not represent our opinion of the phylogenetic pattern but reflect a failure in the time resolution of the available fossil evidence and result from the superimposition of two adjacent dichotomies; their order of occurrence can be suggested on the basis of comparative anatomy. Each point will be discussed using the reference numbers on Fig. 3.

(1) Polychaetes are too specialised to have been ancestral to chordates and *vice versa*, and so the earliest known representative of either group provides evidence that they had diverged. *Spriggina* and *Dickinsonia* (Precambrian/Cambrian transition, Australia)⁴¹ are recognised as possible polychaetes. *Pikaia*, *Miskitoia*, *Canadia* and *Worthenella* (Middle Cambrian, British Columbia) are generally accepted as polychaetes⁴²; although none is undoubtedly this group probably includes the earliest known polychaete. *Palaeoscolex* (Tremadocian, England) was not originally described

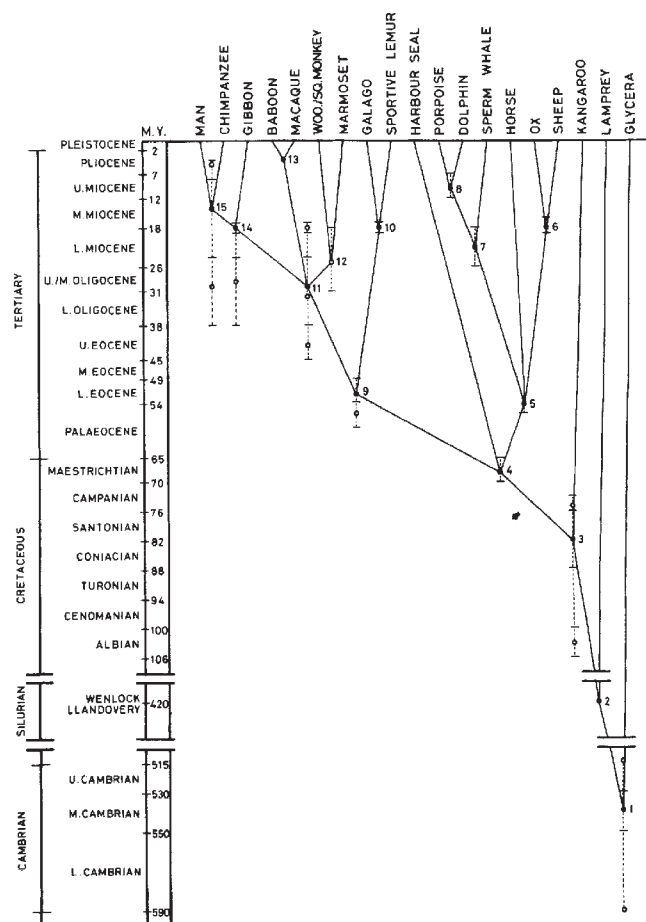


Fig. 3 Phylogenetic chart of the species included in this study, the points of divergence being based on the fossil record and numbered for reference purposes. The most probable point is indicated by a solid circle (●), possible alternatives by open circles (○). Horizontal lines indicate the time range within which a fossil cannot be definitely fixed; where the centre point of this range was used fractions were rounded to the nearest million years older. Some radiometric dates have been placed on the absolute time scale regardless of stratigraphical correlations. *Homo sapiens*, man^{3,8,10,11}; *Pan troglodytes*, chimpanzee¹¹; *Hylobates agilis*, gibbon¹⁰; *Papio anubis*, baboon¹²; *Macaca fascicularis*, macaque¹²; *Lagothrix lagotricha*, woolly monkey^{15,16}; *Saimiri sciureus*, squirrel monkey^{15,16}; *Callithrix jacchus*, marmoset^{15,16}; *Galago crassicaudatus*, galago¹⁷; *Lepilemur mustelinus*, sportive lemur¹⁷; *Phoca vitulina*, harbour seal⁴; *Phocaena phocaena*, porpoise⁴; *Delphinus delphis*, dolphin^{6,9}; *Physeter catodon*, sperm whale⁴; *Equus caballus*, horse²; *Bos taurus*, ox³; *Ovis aries*, sheep^{13,14}; *Macropus giganteus*, kangaroo⁷; *Petromyzon marinus*, lamprey³³; *Glycera dibranchiata*³².

as a polychaete⁴³; it has since been both doubted as an annelid⁴² and regarded as the earliest record of polychaete scolecodonts⁴⁴.

(2) Assuming a monophyletic origin this dichotomy can be equated with the earliest evidence of Gnathostomata, provided by the acanthodian *Nostolepis* (Llandovery/Wenlock, Czechoslovakia)⁴⁵. The extent of stratigraphical uncertainty is not known.

(3) Metatheria and Eutheria diverged from a common therian stem, but it is controversial whether this stem was metatherian or eutherian or neither. The affinities of *Pappotherium* and *Holoclemensia* (Albian, Texas), claimed as eutherian and metatherian respectively⁴⁶, are open to question^{47,48}. *Zalambdalestes* and *Kennalestes* (Bayn Dzak, Mongolia) are undoubtedly eutherians and unsuitable as ancestors to Metatheria. Kielan-Jaworowska has suggested a possible Coniacian/Santonian age for the Bayn Dzak fauna⁴⁹, as indicated on our chart, but McKenna prefers a Cenomanian age⁵⁰. A record of Santonian marsupials in North America⁵¹ is not currently accepted⁴⁷ but marsupials and placentals coexist in the Lower Campanian (Alberta)⁵², and this is shown as a possible point in case Bayn Dzak turns out to be younger than expected.

(4) This point emerges as a trichotomy. Considering first the separation of Primates from the basic eutherian stock, *Purgatorius* (known by many teeth in Lower Palaeocene, Montana, and one tooth from Maestrichtian, Montana) has been claimed as the earliest known primate^{53,54}. This form is little different from several early eutherians, including arctocyonid *Condylarthra*^{55,56}, and its assignment to the Primates has been doubted⁴⁸. It is generally accepted that the order *Condylarthra* was ancestral to Artiodactyla and Perissodactyla and probably to Cetacea. The earliest known condylarthran is *Protungulatum* (Maestrichtian, Montana)⁵⁷. From this, if we accept *Purgatorius* as a primate and *Protungulatum* as an ungulate, both first known at the same time, then the remaining eutherian stock is likely to include the ancestors of the Carnivora. The earliest Carnivora are the miacids *Proictis* and *Ictidopappus* (Middle Palaeocene, North America), although a possible *Ictidopappus* tooth has been reported from Lower Palaeocene⁵⁸. It is, however, possible that miacids were derived from arctocyonids, in which case the phylogenetic chart would have shown a dichotomy between Primates and *Condylarthra* followed by a dichotomy on the condylarthran line giving rise to the Carnivora, but on the present evidence we prefer to remain uncommitted. If *Purgatorius* should prove not to be a primate, then the trichotomy would become a dichotomy between *Condylarthra* and the remaining eutherian stock, which would lead to a Middle Palaeocene dichotomy between Primates and Carnivora.

(5) This point is represented by a trichotomy. Considering first the dichotomy between Artiodactyla and Perissodactyla, it can be defined by the differentiation of either order from the *Condylarthra*. The perissodactyl *Hyracotherium* (Late Palaeocene/Eocene transition, Wyoming)⁵⁹ just antedates several artiodactyls, including *Diacodexis* in the lowest Eocene^{60,61}.

There is a body of opinion that Cetacea may have arisen from mesonychid condylarthrans^{60,62} and it could be argued that differentiation of mesonychids from arctocyonids represents the point of divergence of Cetacea. On the other hand, it is not inconceivable that Cetacea arose from arctocyonids through another intermediate group still unknown. It is thus assumed that when the Artiodactyla and Perissodactyla had diverged from the *Condylarthra*, the remaining condylarthran stock would have included the ancestors of Cetacea. The earliest recorded cetacean is *Angiocetus* (Lower Eocene, England)⁶³.

(6) We have accepted that *Eotragus* (Burdigalian, Europe)⁶⁴ is likely to be connected with the ancestry of oxen and bovine antelopes but is unlikely to have given rise to either

gazelles or sheep⁶⁵. Probable time limits on a radiometric scale are based on Van Couvering's comparative study of the European Neogene⁶⁶.

(7) Among toothed whales (Odontoceti) the earliest indication of dichotomy between sperm whales (Physeteridae) and the remaining stock (including dolphins and the ancestors of porpoises) is provided by *Physeter* itself (Lower Miocene, Europe) and by *Apenophyseter*, *Diaphorocetus* and *Idiorophus* (Lower Miocene, South America)⁶⁷.

(8) We cannot rule out the possibility that dolphins (Delphinidae) were ancestral to porpoises (Phocaenidae), or vice versa, and so their divergence cannot be established until such time as both groups were present. Several dolphins are recorded from the Lower Miocene but the earliest known porpoises are *Palaeophocaena* and *Proto-phocaena* (Upper Miocene, Europe)⁶⁷⁻⁶⁹. If Delphinidae did not give rise to Phocaenidae, then this dichotomy would need to be moved to the point of first appearance of Delphinidae; it would then coincide with the previous dichotomy and form a trichotomy.

(9) Assuming that the suborder Anthropeidea (which includes monkeys, apes and man) arose from the suborder Prosimii (which includes lemurs) then at first sight this dichotomy can be fixed at the earliest record of Anthropeidea. On the other hand, within the Prosimii it is generally accepted that Anthropeidea arose from the infraorder Tarsiiformes. As it is most unlikely that Tarsiiformes gave rise either to the infraorder Lemuriformes or to the infraorder Lorisiformes, (which includes *Galago*) then the dichotomy can be represented best by the earliest known member of the Tarsiiformes (excluding the Paromomyidae, which some authorities now place in the infraorder Plesiadapiformes)⁵⁶. *Berruvius* (Upper Palaeocene, France) and *Navajovius* (Upper Palaeocene, Colorado) are possible candidates, but Szalay regards both as paromomyids⁷⁰. We have indicated Lower Eocene as the most probable point by which time several anaptomorphid tarsoids (including *Tetonius*) were present⁵⁶.

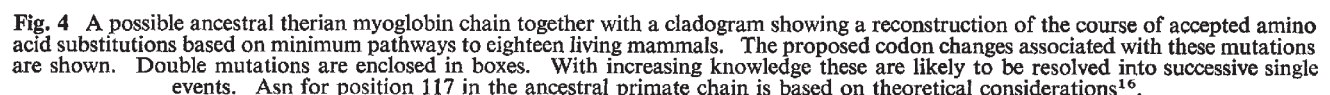
(10) There is general agreement that the Lorisiformes were derived from early members of the Lemuriformes and their divergence cannot be established until such time as both groups were present. Lemuriformes are represented in the Eocene, but Lorisiformes are unknown before *Progalago* and *Komba* (Miocene, Rusinga and Songhor, Kenya)⁷¹, for which radiometric dates are available^{72,73}.

(11) The divergence between the Ceboidea (New World monkeys), the Cercopithecoidea (Old World monkeys) and the Hominoidea (including apes and man) emerges as a trichotomy. The earliest possible point represents the suggestion that *Amphipithecus* and *Pondaungia* (Upper Eocene, Burma) could be catarrhines, perhaps Hominoidea⁵⁶. A second possible point represents the single mandible of *Oligopithecus* (Yale quarry E, Fayum, Egypt) which is probably catarrhine and has been interpreted as a hominoid⁷⁴. The most probable point represents an estimated age for the hominoid *Propliopithecus* and the parapithecine cercopithecid *Apidium* (Yale quarry G, Fayum)⁵⁶.

The remaining tarsoid-anthropoid stock, which was neither hominoid nor cercopithecid, presumably included the ancestors of the Ceboidea, but with the exception of the possible ceboid *Branisella* (Lower Oligocene, Bolivia)⁷⁵, none is known before *Dolichocebus* and *Homunculus* (Upper Oligocene/Lower Miocene, Patagonia)⁷⁶.

It is not universally accepted that *Apidium* and *Parapithecus* are cercopithecid, and so we indicate a radiometric date for the monkey *Victoriapithecus* (Miocene, Rusinga)⁷⁷ as a later possible point. The cercopithecid *Prohylobates* (Miocene, Egypt) represents an alternative to this point but its age is uncertain⁷⁴.

(12) Within the Ceboidea this dichotomy represents the divergence between Callitrichidae and Cebidae. Their relationship is obscure and the possible point is based on



the suggestion that *Dolichocebus* is a callitrichid and *Homunculus* a cebid⁷⁸, but Simons regards both as cebids⁵⁶. In Fig. 3 this is the only dichotomy based on a possible point, but in the absence of other fossil callitrichids we cannot indicate a more probable point. Among the Cebidae none of the few known fossils gives any indication of a possible time of divergence between squirrel monkey and woolly monkey.

(13) This dichotomy represents the divergence of baboons from the less specialised stock which includes macaques. *Dolichopithecus*, *Procynocephalus* and *Libypithecus* have been interpreted as terrestrially adapted macaques rather than baboons⁵⁶, and the gelada *Theropithecus* (= *Simopithecus*) is first known at about the same time as *Parapapio*, which appears to be ancestral to the modern baboon *Papio*⁷⁹. The earliest record of *Parapapio* (Kanapoi Beds, Turkana, Kenya) is dated at about 4 m.y. (ref. 80).

(14) Within the Hominoidea this dichotomy represents the divergence between Hylobatidae (including gibbon) and Pongidae (including chimpanzee), but their relationship is obscure. The earliest possible point represents the suggestion⁸¹ that *Aeolopithecus* (Yale quarry I, Fayum) has hylobatid affinities, in contrast to the better known *Aegyptopithecus* (same site) which is accepted as a pongid. Excluding *Limnopithecus legetet*, now doubted as a hylobatid (P. J. Andrews, personal communication), the most probable point is based on '*Limnopithecus*' *macinnesi* and *Dryopithecus* (*Proconsul*) at Rusinga⁸², the former accepted as a hylobatid and the latter as a pongid.

(15) The earliest possible date represents a minority view, advocated most recently by Kurten⁸³, that *Propliopithecus* is an ancestral hominid. A more widely held view is that the Hominidae arose from within the Pongidae at some later stage. The most probable point represents *Ramapithecus*, which is widely accepted as a hominid. It is recorded from both India and Africa and the most accurately dated find is at Fort Ternan, Kenya⁸⁴, with a radiometric age of 14 m.y. (ref. 73). For this most contentious point we have indicated that which is generally accepted among primatologists as most probable, but we have doubts as to whether sufficient is known about *Ramapithecus* to be sure of its hominid affinity. The earliest undoubted hominid is *Australopithecus* (Lothagam 1 Bed, Kenya) with an estimated date of 5 m.y. (ref. 80).

Reconstruction of Possible Ancestral Therian Myoglobin

Figure 4 shows a cladogram (the pattern of branching being based largely on the phylogenetic chart) indicating how the mutations could have taken place along the different lines leading to the eighteen mammal species, together with a reconstruction of a possible ancestral therian myoglobin. Most of these mutations could be allocated with confidence. In a few cases, however, there was more than one possibility and the amino acid decided on was the most economical from the point of view of the genetic code. The uncertainty is less when species are closely related (baboon-macaque) and increases with distance (kangaroo). As the genetic code is degenerate it was possible to limit the number of codons on several occasions when subsequent mutations were taken into account, assuming single point substitutions in the triplets. Published partial sequences for myoglobin were also considered: dog⁸⁵, badger⁸⁶, humpback whale⁸⁷, pig⁸⁸ and chicken⁸⁹, as well as our preliminary results on the hedgehog and *Tupaia*.

The cladogram shows 147 mutations fixed in at least 82 m.y. of evolution; seventy-four of these are parallel changes which have occurred at twenty-four positions. At some of these positions parallel changes occurred several times: sixteen events have occurred at two positions, eleven events

at three positions, one at four and one at five. Seven detectable double mutations were found; they are at positions 57 Arg in the harbour seal, 85 Asn in the porpoise, 122 Gln in the dolphin, 67 Val in the horse, 9 Ala and 142 Ala in the ox and 74 Asn in the kangaroo. The cladogram also shows seven detectable back mutations, 13 Ile in the branches leading to the cebids and *Galago*, 23 Gly in the pongid branch, 112 Ile in the woolly monkey, 113 Gln in the catarrhine branch, 21 Val in the sportive lemur and 152 Gln in the sperm whale.

The trichotomies shown on the phylogenetic chart (Fig. 3) were resolved into successive dichotomies in the cladogram (Fig. 4) because certain amino acids were shared by two instead of three branches: apes and cercopithecoids have in common residues 66 Val, 86 Ile, 110 Ser and 113 Gln; harbour seal and Condylarthra shared residues 66 Asn, 101 Ile and 116 His; and horse and artiodactyls share residues 34 Thr, 109 Asp and 122 Asn; the branch leading to the dolphin has an immediate common ancestor with the sperm whale, instead of with the porpoise. According to traditional zoological opinion the porpoise and dolphin belong together in the Delphinoidea and the sperm whale belongs to the Physeteroidea. It is then striking that the sperm whale and dolphin share residues 1 Val, 12 His, 15 Ala, 28 Ile and 74 Ala. On the other hand, there is only one residue shared by the porpoise and dolphin, 121 Ala. To join the dolphin with the porpoise instead of with the sperm whale branch requires either parallel evolution of the lines leading to sperm whale and dolphin and an independent fixation of five mutations over a period of not less than 22 m.y., or that these five changes happened after the ancestral cetacean branch separated from the Condylarthra. In the latter case the line leading to the porpoise would have had to accommodate five back mutations. Both these possibilities are unlikely even taking into consideration that powerful selection forces are exerted upon the myoglobin of diving mammals⁹⁰. Comparison of other molecules from the three species will help to resolve this problem.

The ox has the largest number of fixed mutations and one wonders whether this is the result of selection by man, combined with the possibility of genetic drift in small populations⁹¹. A similar phenomenon was, however, not found in the sheep. To resolve this problem we propose to study the myoglobin of wild oxen. It is interesting that a large number of fixed mutations has been claimed for calcitonins in the ancestral artiodactyl stock⁹².

In the palaeontological tree the woolly monkey and squirrel monkey were not distinguished because no fossil evidence has been discovered to date their divergence. However, their common ancestry is indicated by the shared residues 13 Ile and 81 Gln and their later divergence by 66 Val, 112 Ile and 114 Ala in the woolly monkey and 66 Thr and 106 Leu in the squirrel monkey.

Figure 5 shows that the lineages of *Galago* and *Lepilemur* (after they diverged from their common ancestor) appear to have two of the highest mutation rates, but the only fossil evidence documenting their separation is dated at 18 m.y. If the two prosimians diverged from their immediate ancestor not long after it separated from the branch leading to the anthropoids, then this would fit well with the separation of Madagascar from the African mainland, which probably isolated the island's prosimians about 40 m.y. ago⁹³.

The total number of mutations fixed in the different lineages during the past 82 m.y. ranges from 13–28 (mean 19.3). All of these totals (with the marginal exception of that for the ox) fall within two standard deviations from the mean. Although this is consistent with the hypothesis of a constant rate of molecular evolution, it must be emphasised that inability to demonstrate a significant difference from the mean does not prove identity. We are, of course, aware that similar values are to be expected for

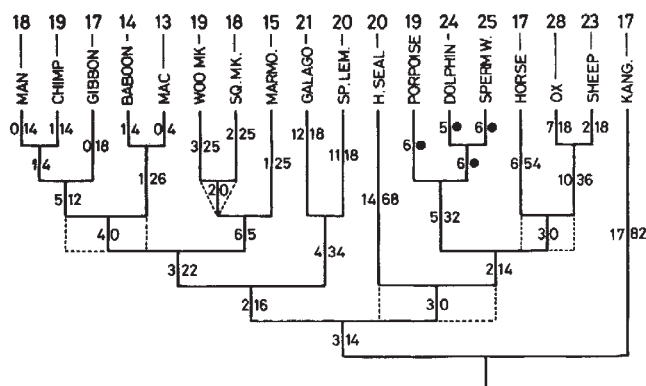


Fig. 5 Cladogram showing on the left of each branch the minimum number of amino acid substitutions, and on the right an estimate of elapsed time based on the most probable points of divergence. The minimum total of accepted mutations is shown for each species since eutherian/metatherian divergence. Double mutations are counted as two steps. As the fossil evidence for the divergence within the Cetacea differed from that used in the cladogram no times (●) are given for this group. The vertical and horizontal broken lines indicate resolved trichotomies.

species which have diverged relatively recently because a large part of their ancestry has followed an identical course. Nevertheless, similar values are obtained for the four groups which diverged more than 68 m.y. ago, the mean value for primates being 17.4, for condylarthran derivatives 22.7, for the harbour seal 20 and for the kangaroo 17.

Although it has been accepted that the rate of evolution of anatomical structures is variable^{94,95} this has been disputed for single molecules⁹⁶⁻¹⁰⁴. Figure 6 shows the relationship between time and the number of fixed mutations along the different branches leading to the living species. We note that periods of apparently low rate of fixed mutations are often followed by apparently high rate episodes, and vice

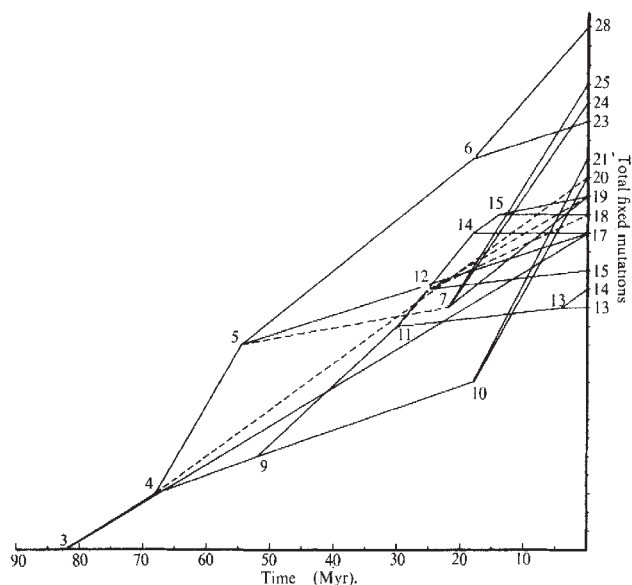


Fig. 6 Relationship between time and the number of fixed mutations along the branches leading to the living species. Numbered points of divergence and pattern of branching as in Fig. 3, except the cetaceans which are shown as a trichotomy because of the different pattern suggested by their myoglobins. The slope of the line reflects the rate at which mutations have been fixed but where trichotomies can be resolved the slope of some lines (broken) is known to be less reliable because the time of divergence differs from the point indicated. Other extreme changes of slope are discussed in the text.

versa; these fluctuations could result in a few cases from the chosen interpretation of the pathway of mutations or they could reflect inaccuracies in the time of divergence, or they could be real. For example, point 10 is probably too late owing to imperfection of the fossil record and points 14 and 15 may be too early owing to problems of interpretation of the fossils.

Differences in the rate of fixation can be demonstrated (Fig. 5) by comparing the number of mutations along two lines rising from the same point, for example, six mutations (horse) compared with twelve (sheep) in 54 m.y. (point 5 in Fig. 6), and similarly seven mutations (chimpanzee) compared with one (macaque) in 30 m.y. (point 11 in Fig. 6). When two lineages arising from the same point differ in their rate of fixation in this way it is self evident that both cannot have the same rate as their common stem and so, apart from differences in rate between lineages, there are also changes in the rate of fixation within lineages.

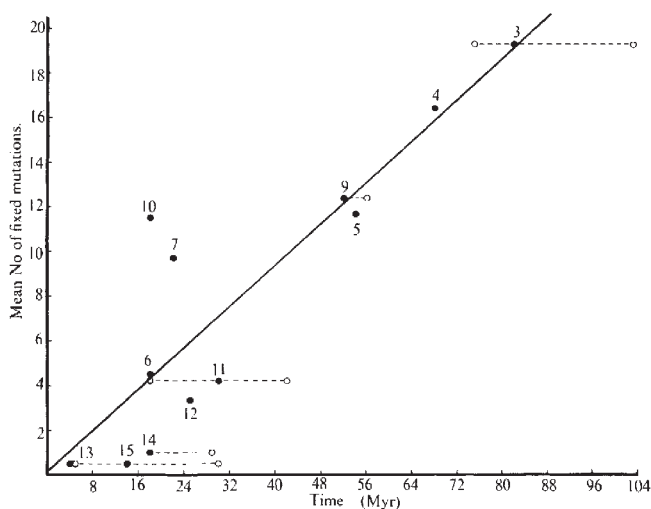


Fig. 7 Relationship between time of divergence and the mean number of fixed mutations in the different lineages since divergence, for eighteen mammalian species. Numbered points and possible alternatives as in Fig. 3. The regression of mutations (Y) on time (X) is given by $Y = 0.23X + 0.10$.

Figure 7 shows the relationship between time of divergence and the mean number of mutations fixed in the different lineages since that time. The wide scatter of points in the region of lower values may be attributable to low mutation values being subject to high sampling errors and to those factors causing differences in rate, both real and apparent, as discussed for Fig. 6. The suspect points of divergence (for example, 10) lie towards the edge of the scatter, whereas differences in rate between lineages arising from one point are more likely to cancel one another out. Thus, point 6, which falls near the fitted line, results from averaging seven mutations (ox) and two (sheep).

The coefficient of correlation for the numbered points in Fig. 7 is 0.89, but this value is spuriously high because each common ancestral stem contributes a constant value towards the total for each of its descendant branches, and numerically lower values are necessarily components of the higher. Bearing in mind this self reinforcement of the data, it can be seen that a regression line fitted to the plotted points suggests a rate of one fixed mutation in about 4 m.y. On the other hand, fluctuations in rate within a lineage can be expected to average out over longer periods of time giving an overall rate for that lineage, and different overall rates can themselves be averaged. As can be deduced from Figs 5 and 6, considering the last 82 m.y., the average rate

for the eighteen mammalian myoglobins is one mutation fixed in 4.2 m.y., whereas the overall rate for the lineage leading to sperm whale is 1 in 3.3, and to macaque is 1 in 6.3. Thus, in the context of molecular evolution we believe that average rate is a more appropriate term than constant rate, and that overall rates for separate lineages are likely to be more informative than the average rate.

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Is the Universe a Vacuum Fluctuation?

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The author proposes a big bang model in which our Universe is a fluctuation of the vacuum, in the sense of quantum field theory. The model predicts a Universe which is homogeneous, isotropic and closed, and consists equally of matter and anti-matter. All these predictions are supported by, or consistent with, present observations.

RECENT observations confirm that the 2.7 K background radiation does indeed have a blackbody spectrum (see refs 1 and 2), which establishes beyond reasonable doubt that some version of the big bang theory is correct. Here I propose a specific big bang model which I believe to be the simplest and most appealing imaginable—namely, that our Universe is a fluctuation of the vacuum, where ‘vacuum fluctuation’ is to be understood in the sense of quantum field theory.

Creation

In any big bang model, one must deal with the problem of ‘creation’. This problem has two aspects. One is that the conservation laws of physics forbid the creation of something from nothing. The other is that even if the conservation laws were inapplicable at the moment of creation, there is no apparent reason for such an event to occur.

The prevailing attitude towards creation is that our Universe is probably undergoing merely one in an infinite sequence of expansions (with intervening contractions). According to this view the Universe has always existed, so its origin lies in the infinite past where it is hopefully not a problem for science.

The preceding viewpoint may indeed be correct, but it leaves unanswered two important questions. First, by what mechanism does the Universe ‘bounce back’ from each contraction? No satisfactory mechanism has ever been proposed. Second, why does the Universe have its particular values for energy, electric charge, baryon and lepton number and so on?

In my model, I assume that our Universe did indeed appear from nowhere about 10^{10} yr ago. Contrary to widespread belief, such an event need not have violated any of the conventional laws of physics. The laws of physics merely imply that a Universe which appears from nowhere must have certain specific properties. In particular, such a Universe must have a zero net value for all conserved quantities.

Conservation

The conserved quantities of physics fall into two categories—discrete and continuous. The discrete quantities are those which characterise elementary particles: electric charge, baryon and lepton number, strangeness and so on. All these quantities have equal magnitude but opposite sign for particles and anti-particles. Hence the discrete conservation laws simply imply that a universe which appears from nowhere must consist equally of matter and anti-matter. Such a possibility is consistent with the properties which have so far been established for our own Universe. (The possibility that our

Universe consists equally of matter and anti-matter has been studied by many authors; for a recent review see ref. 3.)

Of the remaining conservation laws, the most important for cosmology is that concerning energy: although matter and energy can be converted into each other, the net energy remains constant if an intrinsic energy of mc^2 is assigned to each piece of matter.

The Universe has an enormous amount of mass energy, and this might be thought to preclude a creation of the cosmos from nothing. There is, however, another form of energy which is important for cosmology, namely gravitational potential energy. The gravitational energy of a mass m due to its interaction with the rest of the Universe is given roughly by

$$E_g \approx -GmM/R$$

where G is the gravitational constant and M denotes the net mass of the Universe contained within the Hubble radius $R=c/H$, where H is Hubble’s constant.

The density of matter which has so far been observed is somewhat less than the critical value ρ_c required for the Universe to be closed:

$$\rho_c = 3H^2/8\pi G$$

Sandage’s recent determination⁴ of the cosmic deceleration parameter indicates, however, that our Universe probably is closed, in which case the true ρ exceeds ρ_c . If I assume the critical density in my estimate of E_g , I obtain

$$E_g \approx -mc^2/2$$

Hence within a factor of order unity, the negative gravitational energy of any piece of matter is sufficient to cancel the positive mass energy of mc^2 . This naive argument indicates that the net energy of our Universe may indeed be zero.

P. Bergmann has presented a more sophisticated argument which indicates that any closed universe has zero energy. In its simplest form, the argument proceeds as follows (J. M. Cohen, private communication).

Suppose the Universe were closed. Then it would be topologically impossible for any gravitational flux lines to escape. If the Universe were viewed from the outside, by a viewer in some larger space in which the Universe were imbedded, the absence of gravitational flux would imply that the system had zero energy. Hence any closed universe has zero energy.

The preceding remarks indicate that our Universe may have zero net values for all conserved quantities. If this be the case, then our Universe could have appeared from nowhere without violating any conservation laws.

Quantum Field Theory

To indicate how such a creation might have come about, I refer to quantum field theory, in which every phenomenon that could happen in principle actually does happen occasionally in practice, on a statistically random basis. For example, quantum electrodynamics reveals that an electron, positron and photon occasionally emerge spontaneously from a perfect vacuum. When this happens, the three particles exist for a brief time, and then annihilate each other, leaving no trace behind. (Energy conservation is violated, but only for

the brief particle lifetime Δt permitted by the uncertainty relation $\Delta E \Delta t \sim \hbar$, where ΔE is the net energy of the particles and $\hbar$ is Planck's constant.) The spontaneous, temporary emergence of particles from a vacuum is called a vacuum fluctuation, and is utterly commonplace in quantum field theory.

If it is true that our Universe has a zero net value for all conserved quantities, then it may simply be a fluctuation of the vacuum, the vacuum of some larger space in which our Universe is imbedded. In answer to the question of why it happened, I offer the modest proposal that our Universe is simply one of those things which happen from time to time.

One might wonder how a vacuum fluctuation could occur on such a grand scale. My answer is in two parts. The first is that the laws of physics place no limit on the scale of vacuum fluctuations. The duration is of course subject to the restriction $\Delta E \Delta t \sim \hbar$, but this merely implies that our Universe has zero energy, which has already been made plausible.

The second part of my answer lies in the principle of biological selection, which states that any Universe in which sentient beings find themselves is necessarily hospitable to sentient beings. I do not claim that universes like ours occur frequently, merely that the expected frequency is non-zero. Vacuum fluctuations on the scale of our Universe are probably quite rare. The logic of the situation dictates, however, that observers always find themselves in universes capable of generating life, and such universes are impressively large. (We could not have seen this universe if its expansion-contraction time had been less than the 10^{10} yr required for *Homo sapiens* to evolve.)

In summary, observations imply that a big bang occurred about 10^{10} yr ago. One might suppose that the Universe has always existed, undergoing periodic expansions and contractions. There is, however, no known mechanism by which the Universe might bounce back from a contraction. Furthermore, to assume the Universe is infinitely old is to evade, rather than illuminate, the issues of its origin and quantum numbers.

I assume the Universe to be undergoing its initial expansion, evidently having appeared as a fluctuation of the vacuum. The conservation laws of physics then imply that our Universe has the quantum numbers of the vacuum, including zero energy. Hence our Universe must be homogeneous, isotropic and closed, and must consist equally of matter and anti-matter. All these predictions are supported by, or consistent with, present observations.

My model is admittedly speculative, and is still in an early stage of development. Quantum theory does, however, imply that the vacuum should be unstable against large scale fluctuations in the presence of a long range, negative energy, universal interaction. Gravitation is precisely such an interaction, so I am encouraged to believe that the origin and properties of our Universe may be explicable within the framework of conventional science, along the lines indicated here.

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Peace Research and SIPRI

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Peace research only began in earnest twenty years ago and the Stockholm International Peace Research Institute was founded as recently as 1966.

In 1945, Einstein said that the atomic bomb "may intimidate the human race into bringing order into its international affairs, which, without the pressure of fear, it would not do". Einstein was wrong. He did not anticipate that the human race would become rapidly injured to the danger of its own destruction. Twenty years later President Kennedy, seriously alarmed by the general antipathy to the dangers inherent in the deployment of tens of thousands of nuclear weapons, warned that the greatest risk of a general nuclear war—by then capable of eliminating human life on the Earth—was not through calculated intention, but through miscalculation, madness or accident. This year, events during the fourth Middle East war dramatically demonstrated both the truth of President Kennedy's sombre statement and the degree of disorder now existing in international affairs.

Clearly, neither the United States nor the Soviet Union actually desires a nuclear confrontation. And yet the indication that Soviet troops might have been on the way to the Suez Canal led to a world-wide American military alert. The Soviet leaders' miscalculation of the possible American reaction and the American President's actual over-reaction (how

much influenced by domestic political events we will probably never know) to the Soviet move brought the world to the edge of the thermonuclear abyss for the second time in a decade—the Cuban missile crisis being the first. And this nuclear confrontation occurred during a period of *détente*. Can it be reasonably doubted that in some future nuclear confrontation, one or other side will not be deterred by, or will seriously misinterpret, the other's moves? What will happen if a future crisis in a tension area occurs when both leaders of the great powers are severely stressed by domestic pressures? And how much more likely will nuclear war by miscalculation, accident or madness become when many more than the present five countries have nuclear weapons? Is it not apparent that disorder in international affairs will continue to increase unless steps are taken to reverse the trend?

Peace Research

Concern over the consequences of increasing disorder in international affairs and the consequent increase in violence, of the increasing role of military force in international relations, of the increasing probability of nuclear holocaust, of the increasing inhumanity of conventional weapons and methods of warfare, and of the increasing dangers inherent in the world's arms races are some of the objective factors which motivate scientists to work on peace research.

By far the major part of today's science and technology is funded from military expenditures. The importance attached

by states to military strength leads to a massive investment in military research and development, an investment much greater than that put into research and development for peaceful purposes. By comparison with military spending, the resources devoted to understanding the reasons for, and the nature of, international conflicts are minute. So minute, in fact, that many scientists are unaware of the existence of peace research, let alone the results of this research.

The origins of peace research can be traced back to two main sources—the work of the American Quincy Wright on the nature of war and that of Lewis Richardson, a British meteorologist, who developed in the 1930s (although his work remained virtually unknown until 1960) a remarkable mathematical theory of the international system and arms races. It was, however, only about twenty years ago that peace research really began to get under way. The relative infancy of the discipline is shown by the fact that it was not until its 1973 conference at Montreal that the International Political Science Association decided that the time had come to establish a peace research committee to define the subject and to coordinate activity within the field.

Scientists working in peace research generally belong to one of two groups. One group argues that the existing international system, based on a large number of sovereign states, is inherently unstable and will inevitably lead to increasingly severe conflicts and probably eventually to the nuclear destruction of mankind. The only solution, therefore, is to create an entirely new system. Insofar as this group studies the present international system at all, it is to find ways of radically changing it. Research, therefore, focuses on the development of a body of theories and models both about the present international system in an attempt to pinpoint the weaknesses of it and, more often, about possible new international systems which, so it is hoped, will be more stable and less costly. To this end, very complex theories have been developed and analysed, based on game theory, communications theory, systems analysis, cybernetics and so on.

The second group takes the present international system for granted and attempts to discover how it works, to suggest ways of operating it more successfully. It is assumed that by the application of traditional methods—multilateral and bilateral negotiations between states, the development of international law, the United Nations, high level diplomacy and so on—the international system can be made to evolve into an increasingly stable and thus more peaceful system, provided only that the political will to achieve this end exists. The increasing interdependence of states, for example, is seen as an inevitable result of the natural evolution of the present international system. The development of the international system will come about mainly through the efforts of the world's policy makers, who in turn will be influenced by that small group of persons who mould public opinion. And it is, therefore, to the policy makers and the moulders of public opinion that the second group of peace researchers aim the results of their research.

The procedure adopted is to identify, in general terms, those areas in which policy makers could bring about beneficial changes in the international system. Which problems is it politically realistic to expect policy makers to deal with at a given time, bearing in mind the present state of international relations? What are the consequences if a given problem is not solved? With which objectives in mind should particular changes be made to produce the most beneficial effects?

Both approaches to peace research have undoubtedly produced valuable insights into ways of increasing world stability, of improving social justice and of decreasing violence.

Stockholm Institute

The work of the Stockholm International Peace Research Institute (SIPRI), founded in 1966 by the Swedish Parliament to commemorate Sweden's 150 years of unbroken peace, is

entirely within the second of the two categories described above. The established international system is accepted as the framework in which research problems are chosen. No other types of systems are considered. The research is problem oriented and mainly aimed at directly influencing policy makers in current international politics and leaders of public opinion.

An eight-member international governing board is entirely responsible for the operations of SIPRI, even though it is financed by the Swedish Parliament. In the early days, the board decided that the work of the institute should initially concentrate on the problems arising from armaments and disarmament. These were, and still are, considered to be the most serious and urgent of the world's problems. It was soon apparent to the first director of the institute, the British economist Robert Neild, that the most urgent requirement of policy makers in the field of disarmament was relevant data, objectively presented and analysed by an international group of scientists, particularly on the scientific and technical issues involved in disarmament. All of the participants at, for example, the Conference of the Committee on Disarmament (CCD) in Geneva, the main international forum for disarmament negotiations, were, and are, in need of such information, although the delegates of the smaller powers, who generally lack scientific advisers, have the greatest need. The institute, therefore, began by collecting and publishing data on armaments and on developments in military technology, and by analysing ongoing disarmament and arms control negotiations.

One of the first subjects studied in depth at SIPRI was chemical and biological warfare (CBW). In 1964 a group of microbiologists, seriously concerned about the problems of biological warfare and the consequences of the possible military uses of new advances in biology, began meeting as a Pugwash Study Group. It soon became evident that there was a need for a more intensive study than this group could hope to achieve by occasional meetings; SIPRI, therefore, decided to undertake a major review of biological warfare but the study was soon extended to cover chemical warfare as well. The work led to the publication of a six-volume study, *The Problem of Chemical and Biological Warfare*, dealing with the historical, technical, military, legal and political aspects of CBW and possible disarmament measures.

Another subject taken up for study was the trade in major weapons—ships, aircraft, tanks and missiles—between the developed and the non-developed countries during the period after the Second World War. Although the international trade in arms had received much attention in the 1930s, it had been largely ignored in the post-war period. A main aim of the SIPRI study was to propose measures to limit this important element of the global arms race.

When the Partial Test Ban Treaty, prohibiting nuclear weapon tests above ground, was signed in 1963 no agreement was reached on the means of verifying a ban on nuclear tests underground. The main stumbling block was whether seismic verification at long range was alone sufficient. The rate of underground nuclear testing increased considerably after the treaty was signed and, in 1968, SIPRI decided to assemble a group of seismologists to examine the advances in techniques of seismic verification and to see what could be accomplished.

Until 1968, it had been generally assumed that it was impossible to distinguish between the seismic signals generated by earthquakes and explosions. If on past experience an event detected on seismographs was probably not an earthquake and was at a shallow depth, then an explosion was suspected. Analysis therefore consisted of diagnosing earthquakes and eliminating them, leaving a small residue of dubious events. At the expert group meeting, however, it was accepted for the first time, by leading seismologists from ten countries (East and West), that signals generated by explosions could, by analysis of their component waves,

be distinguished from the signals generated by earthquakes. Underground nuclear explosions could be thus identified as well as detected. This advance put an entirely new complexion on the possibilities of seismic verification.

The group reported that, with the Western seismic network then existing (which was not specifically designed for detecting underground nuclear explosions) identification down to 20 to 60 kton exploded in granite could be made. But with better equipment (now available) this level could be reduced to 10 kton in granite and, with a wider distribution of sophisticated instruments, the group foresaw the possibility of identification within a range of 2 to 10 kton in granite. The seismic group report, updated in 1970 and 1971 by two progress reports, has had a considerable influence on the ongoing negotiations for a comprehensive nuclear test ban.

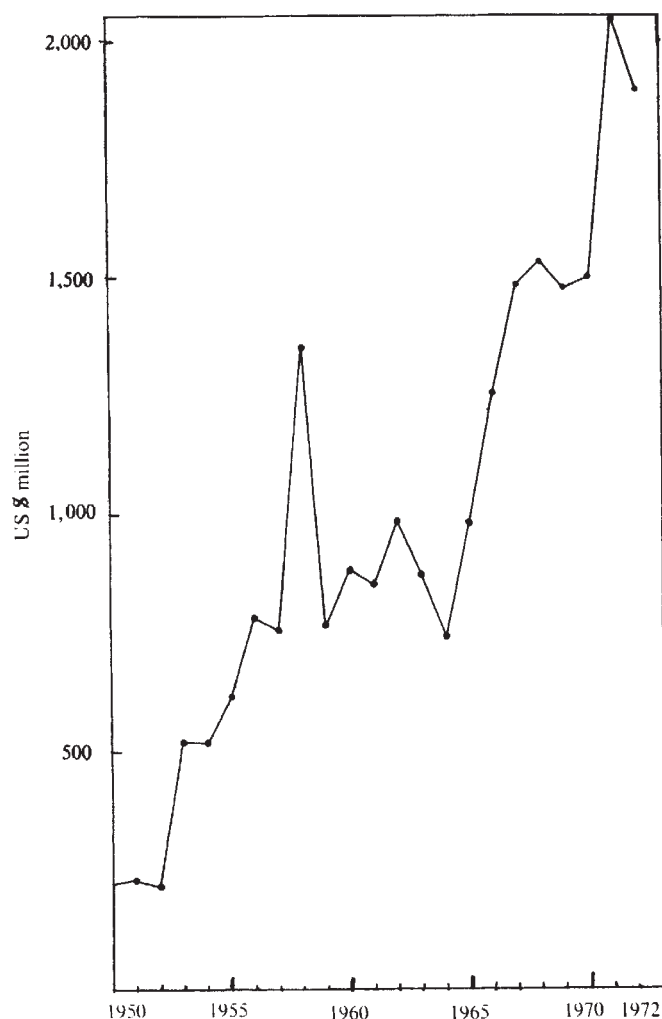


Fig. 1 The arms trade in major weapons between developed and underdeveloped countries 1950–72. (Figures in constant 1968 prices.)

The success of this enterprise led to the formation, in 1971, of another expert group to assess techniques for the inspection of the production of organophosphorus compounds to assist negotiations for a ban on the production of chemical weapons. Once again, the information in the reports arising from these discussions has been used during the negotiations for a CW treaty. The institute is now widening its study of verification issues and is, for example, investigating the potential of established and new technologies for the verification of a range of possible disarmament treaties, up to and including general and complete disarmament.

A third expert group was assembled last June to discuss the military consequences of the spread of peaceful nuclear technology, with particular reference to the issues likely to be raised at the 1975 conference which is to review the operation of the non-proliferation treaty. Is the safeguards system established by the treaty to detect diversion of nuclear material from peaceful to military uses operating satisfactorily? What are the military consequences of new developments in nuclear technology? Does the treaty encourage international cooperation in the development of peaceful nuclear technology to the extent envisaged? Should peaceful nuclear explosives technology be actively developed? If so, under what international regime should this technology be operated? These questions will be dealt with in a two-volume publication; also included will be a set of proposals for the NPT review conference.

The scientists working at SIPRI come from a wide range of social and political backgrounds. Nevertheless, there is general agreement amongst them that the onward rush of military technology will sooner or later produce a catastrophic war and that the only way to eliminate this possibility is by far-reaching disarmament measures. General and complete disarmament is regarded as a politically realistic goal which should be energetically pursued by the world's political leaders. These views are reflected in the SIPRI yearbooks—yearly publications of reference material on world military expenditures, the arms trade, domestic arms production, the nuclear and conventional arms races, current disarmament negotiations and topical issues on armaments and disarmament. The fifth yearbook, to be published next June, will complete the publication of available data on the trade of major arms between the developed countries and ninety-five underdeveloped countries, the arms trade between developed countries, and domestic defence production in developed and underdeveloped countries. The yearbooks are also comprehensive sources of information on bilateral and multilateral disarmament negotiations in all forums.

What general conclusions can be drawn from the activities and publications of SIPRI? In the early 1960s, the decision was taken to abandon attempts to negotiate directly general and complete disarmament and to work instead for so-called partial arms control measures. The theory was that, by this method, it would be possible to move towards general disarmament by small steps. But, even though there have been seven multilateral arms control treaties negotiated since that time, there has been very little progress towards actual disarmament. In addition to the multilateral treaties there have been a number of bilateral agreements between the United States and the Soviet Union. The most important and well known of these are the SALT (Strategic Arms Limitation Talks) agreements—a treaty to limit antiballistic missile systems and an interim agreement on certain measures with respect to the limitation of strategic offensive arms. The political significance of these agreements between the two great powers is considerable but they have been over-advertised as disarmament measures. In fact, both agreements allow quantitative increases in the present level of nuclear weapons since higher ceilings have been established. More seriously, the number of nuclear weapons carried by each missile is allowed to increase unhindered. And there is no limitation at all on the qualitative development of any strategic weapon systems or, for that matter, of any other armaments. For these reasons the SALT agreements can even be regarded as legitimising and encouraging military technological advances. One positive and important disarmament aspect of the SALT agreements concerns the problem of verification. It is noteworthy that the problem of verification, which has plagued all previous disarmament negotiations, was overcome in the SALT agreement, with relative ease. Each party will use its own "national technical means" of verification, chiefly reconnaissance satellites, to provide assurance of compliance.

Prospects for Disarmament

Disarmament in the true sense of the word seems to be fast becoming a dead issue. Even the two obvious next steps, a comprehensive test-ban treaty and a chemical weapons treaty, are unlikely to be soon negotiated. And the current SALT negotiations and the discussions on mutual force reductions in Europe are unlikely to produce far-reaching results for some time to come. Some people are pinning their hopes for further progress on a world disarmament conference. Proposals for such a conference have been made at the United Nations but are stalled by the unwillingness of China to participate. There is, however, reason to assume that China is willing to discuss measures of actual nuclear disarmament given a reorganisation of present negotiating forums. A world disarmament conference could put new life into disarmament discussions. But in itself it would not solve the present problems. The basic requirement is that the nuclear-weapon powers genuinely desire to abolish all weapons of mass destruction as a first step to general disarmament. Once the political will exists, the forum for negotiation is irrelevant.

The recognition of the difficulties of negotiating significant disarmament agreement by the partial-measure approach has led to considerations of other methods of reducing the scope and effect of military operations. One such alternative is the attempt to prohibit firstly the use of unnecessarily cruel and indiscriminate weapons and secondly acts of warfare

directed against the civilian population. The Hague Conventions of 1899 and 1907 prohibit, for example, the use of dum-dum bullets. And the Geneva Protocol of 1925 prohibits the use of gas and other chemical agents and bacteriological agents. But, since these early treaties, enormous improvements have been made in the effectiveness of weapons to kill and maim soldiers and civilians. Initiatives are now being taken to modernise the laws of war to take into account the advances of military technology. For example, the problem of the use of napalm and other incendiary weapons has been put on the agenda of the United Nations. And the Red Cross is considering the possibility of prohibiting the use of a wide range of antipersonnel weapons including high-velocity bullets (which produce wounds similar to dum-dum bullets) fragmentation weapons, etc. The Red Cross may also soon consider such indiscriminate methods of warfare as the terror bombing of civilians, the large scale destruction of food crops, the deliberate flooding of extensive areas and so on. To assist these important initiatives, SIPRI is undertaking a major study of incendiary weapons and weapons which produce effects similar to those which are already banned by international law.

To some extent the lack of progress towards disarmament has disillusioned some of the researchers specialising in the subject. But the great majority are working on, convinced that if the awesome consequences of the increasing militarisation of the world can be made widely enough known the policy makers will have to act.

Comet Kohoutek

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Although comet Kohoutek has not lived up to some of the more extravagant claims made for its astronomical prominence, Professor Biermann shows that observations will still be fruitful because so little is known about the nature of comets.

COMET Kohoutek was discovered on March 7, 1973, near the orbit of Jupiter with a magnitude $m=16$ (or somewhat brighter); an earlier picture was subsequently found on a plate taken on January 28, 1973, at which date the comet was at a heliocentric distance around 5 AU and also of magnitude about 16. The near-parabolic orbit (eccentricity between 0.9999 and 1) was found to have an inclination of 14.3° with a perihelion distance of only 0.14 AU. A comparison with other comets with similar perihelia and similar brightness at large distance from the Sun led to the prediction that near perihelion (which will be passed on December 28, 1973) comet Kohoutek should become at least as bright as Venus, possibly being visible in full daylight.

Comets which, like comet Kohoutek, approach the Sun for the first time, after spending the past 4.5×10^9 yr most probably in the outer fringes of the Solar System, are generally considered to be the oldest essentially unchanged specimens of the products of condensation in the solar nebula, which nearer to the Sun led to the formation of the terrestrial planets or, at somewhat larger distances, of the major planets. For this reason such comets are likely to provide important clues about the very early history of the Solar System and the way in which it formed from collapsing interstellar matter.

The almost star-like central condensation of light seen on plates taken with long-focus instruments, when a comet is far from the Sun and the coma is only weak, is photometrically similar to light from a planetary body¹. To the extent that the influence of the phase function can be neglected (for example, around opposition), the brightness is given essentially by the product of the albedo (A) and the cross section of the nucleus. This cross section, measured for many recent comets by Roemer, was of the order of one or a few km^2 only, the giant in her list of 1956 being comet Humason (1962 VIII), approximately 200 km^2 . For the recent comet Bennett a value of roughly 30 km^2 may be derived from her measurements (see Delsemme²). For comet Kohoutek, measurements made

earlier this year indicate a value of the product of A and cross section about four times that pertaining to comet Bennett, indicating that comet Kohoutek is intermediate in its intrinsic size between comet Bennett and comet Humason, but perhaps more similar to comet Humason. The latter comet, however, already displayed a gas tail at the distance of Jupiter and little dust, whereas comet Kohoutek appears to be rather dusty, with no gas (that is plasma) tail having been discovered so far; CN, a constituent of the coma, was observed on October 15, when the comet was at about 1.8 AU from the Sun.

Icy-conglomerate Model

For the purpose of the present discussion I have adopted in essence the hypothesis embodied in the icy-conglomerate model³. At present the most convincing reason for doing so is the fact that in those cases in which the mass of dust released was quantitatively determined (at solar distance of order unity), it was found to be similar to the gas output. Since these comets were rather dusty, it seems likely that for most comets there is even more gas released than dust. This strongly suggests that the gas is present in the central parts of the coma in the form of ice, presumably in a single block. It is of course true that figures for the dust and the integral gas production are so far available for only very few comets. The resulting diameters are again of the order of 1 to 10 km. This means that, barring very exceptional circumstances, the cometary nuclei cannot be seen as disks of finite angular extent, and the ultimate proof that the nucleus of a typical comet is really a single body made up of ice and dust can only be provided by a space mission towards the comet. Such missions are under discussion, but none has been scheduled so far.

Under these circumstances the observations of comet Kohoutek are likely to provide another value for the ratio of gas to dust, but no ultimate answer to the question of the constitution of the cometary nuclei.

Comet Kohoutek will, however, be the first comet to be observed with adequate instrumentation in almost every important range of wavelength. Observations in the ultraviolet have been made over the past three years of comets Tago-Sato-Kosaka, Bennett, Encke and Toba, and such observations will also be made of comet Kohoutek. They should reveal the extent of the hydrogen coma and the amount of hydrogen released per second. They should furthermore reveal the amount of hydroxyl (OH) molecules, which may then be compared with the amount of hydrogen to find out what fraction of the atomic hydrogen may be ascribed to the dissociation of ordinary water molecules. At solar distances less than about 2 AU it is possible, for reasons that are not at all obvious, that ordinary water will turn out to be the main parent molecules of the hydrogen atoms. Comet Kohoutek is a genuine new comet which must have come from a distance of the order of 10,000 or several tens of thousands of AU, whereas comet Bennett was in an orbit of revolution period less than 2,000 yr. Comet Tago-Sato-Kosaka, on the other hand, was also a newcomer, though less dusty than comet Bennett. In comet Kohoutek a dust halo was seen at a distance from the Sun at which ordinary water would have effectively no vapour pressure. The dust particles seen at $\geq 10,000$ km from the cometary nucleus must therefore have been thrown off by some more volatile gas, the simplest alternatives being methane (CH_4), carbon monoxide (CO) and formaldehyde (H_2CO), or possibly ammonia (NH_3). Methane and NH_3 are chemically the most stable compounds in a hydrogen-rich atmosphere at low temperatures; CO, on the other hand, is by far the most abundant molecule in interstellar clouds, with the exception of molecular hydrogen (H_2). The properties of the cometary plasma suggest that neutral CO

should also be relatively abundant in comets⁴. The question whether the chemically most stable molecules— H_2O is of course also in this class—make up most of the cometary ices, or whether molecules such as CO and H_2O are present in appreciable quantities, is evidently of fundamental importance for the chemistry of comets.

Carbon monoxide molecules can be detected in three different spectral regions: in the microwave range at 2.6 mm, in the infrared at $4.6 \mu\text{m}$ for instance, and with greater difficulty in the ultraviolet. Attempts to detect CO, particularly in the first two wavelength regions mentioned, will be made for comet Kohoutek by several groups in the United States and elsewhere. The observations of CH_4 will be made using aircraft flying at above 40,000 foot. A transition near $3.3 \mu\text{m}$ seems most promising.

It is unfortunately conceivable that most of the volatile material in the surface layers will evaporate before the comet comes within 1 AU or less of the Sun, during which time most of the observations are scheduled to be made. For this reason the outcome of these attempts is somewhat difficult to predict, and in any case the search for volatile substances should begin as early as possible. After perihelion passage, the chances may be even worse.

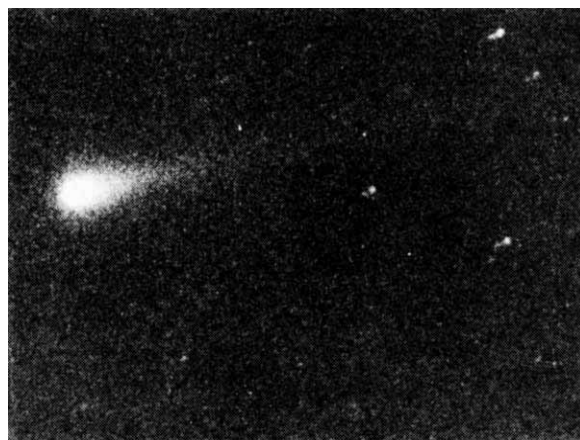


Fig. 1 Photograph of Comet Kohoutek taken at the Royal Greenwich Observatory. On the same scale, the diameter of the Full Moon would be 5.4 cm.

In the radiofrequency range, searches will be made for a number of molecules, in particular formaldehyde at 6 cm, the so-called X-ogen (at 3.3 mm) and OH at 18 cm. The search for the X-ogen molecules, evidenced by an unidentified transition at 89,190 MHz observed so far in about ten interstellar clouds, is of particular interest for the following reason. An identification which seems promising at present is that with the ion of the radical HCO, which occurs in hydrocarbon flames and would be expected to be produced by the (exothermic) reaction of CO ions with molecules containing hydrogen. Bright comets generally show a well developed plasma tail, usually containing many CO ions, and so large numbers of such ions are probably present in comet Kohoutek. If the X-ogen transition were to be observed in comet Kohoutek, the identification mentioned—at this stage only one of several possibilities—would be considerably strengthened.

It goes without saying that observations in the ordinary optical range similar to those which have been made in the past for most bright comets are planned all over the world. Surface photometry of the dust tail, if carried out over a large part of the orbit within 1 or 2 AU, will make it possible to derive both the production rate of dust particles

and the integral gas production, as mentioned already. The technique was developed to a high degree of perfection by Finson and Probst⁵ after the appearance of the two bright comets of 1957. From the isophotes it is possible to derive the initial velocity of the dust particles with respect to the cometary nucleus, that is, the velocity (of the order of 100 or a few hundred metres per second) which the dust particles gain from the drag of the gas which streams out from the surface by evaporation. The analysis shows that this velocity is gained relatively close to the nucleus, because the flux of gas molecules decreases approximately as the inverse square of the distance. In this way the mass flux of the gas can also be determined and has been found to be of the order expected from the absorbed solar radiation and the surface of the comet. The albedo of the cometary nucleus is one of the parameters to be determined; recent work by Delsemme² suggests that the value of the albedo is considerably larger than has generally been assumed until recently, say of the order of several tens of per cent, but reliable values are not yet available. In dusty comets part of the solar light is absorbed before reaching the nucleus to an extent which is difficult to determine, and in addition there are of course dust particles of different sizes for which the ratio of the light pressure to gravity varies with the inverse radius. Comet Kohoutek will provide an excellent opportunity to gain further insights into these rather complex problems.

The only constituent which is at least in some measure representative of the bulk gas production and which is observable in the ordinary optical range is atomic oxygen. The transition in question is the red night sky line which has been seen in almost every bright comet, though accurate photometry has never been carried out owing to the difficulty of separating it from the background. This can be done using the Doppler shift, and it should not be too difficult in the case of comet Kohoutek. The transition probability for this (forbidden) line is so low that photo-excitation would be quite insufficient to explain the observed intensities. The answer to this problem seems to be that the excitation occurs in the process of dissociation or dissociative recombination, which leads to the appearance of the oxygen atom. It is to be hoped that for comet Kohoutek an accurate photometry of the red [OI] line will shed further light on these problems.

The emissions of the neutral molecules and radicals C_2 , C_3 , CN and OH are important in providing clues about the nature of their parents. If their source turns out to be essentially the nucleus itself—they could be contained in ordinary ice in the form of clathrate as suggested many years ago by Delsemme and Swings⁶—the density would, for constant velocity of outflow ($\approx 1 \text{ km s}^{-1}$), vary as the inverse square of the distance from the nucleus, whereas the projected density would vary as the inverse first power of the distance. The expected dissociation at distances between several tens of thousands and several hundreds of thousands of kilometres from the nucleus would give rise to an exponential term and under such simple circumstances the scale distance of the dissociation could be derived from the observations. In principle, it is obviously possible to derive also the scale distance pertaining to the parent molecules, provided that the observed molecule does not emanate from the nucleus itself as suggested above.

In reality such measurements are very hard to make with sufficient accuracy, and it looks as though measurements from space vehicles⁷ yield a superior accuracy because of the background effects which are difficult to remove in measurements made from the ground. The fact that for comet Kohoutek such measurements can be planned sufficiently ahead of time, and that the expected brightness of the comet makes the background problems less serious, justifies the hope that the observations of comet Kohoutek will be a major step in the further elucidation of these problems.

The case of the hydroxyl (OH) radical is special for two reasons. First, because its parent is most probably ordinary water which for obvious reasons cannot be observed from the ground and, second, because the emission is so close to the ozone limit that measurements from space vehicles are probably superior.

Plasma Tails

The plasma tails of comets are of particular interest for several reasons. As was recognised long ago, the rapid motion away from the Sun is due to the transfer of momentum from that component of the Sun's corpuscular radiation now called the solar wind. The details of this interaction are not yet sufficiently well understood, particularly because the magnetic fields embedded in the solar wind are likely to play a decisive role. Theoretical work on these questions has long been in progress and leads to the idea that a bow shock and a kind of contact surface should exist around a comet. Without good new observations, however, it will not be possible to arrive at a full understanding of the problems of the plasma flow around and near a comet. For this reason plans have been made to observe comet Kohoutek at intervals of, say, a quarter or a half an hour at stations of different longitude, so that a continuous record over an appreciable fraction of a day is obtained. To ensure the success of such a programme, it is necessary not only to overcome periods during which observing conditions are poor, but also to gain the cooperation of observatories located on adjacent longitudes separated by not much more than 15° since the comet will be observable from any one station only for about an hour, because of its small elongation angle from the Sun (30° to 40°).

Although such observations will yield valuable information on the dynamical processes occurring in the tail, the origin of the tail plasma can be investigated only by observing the light of one of the emission lines of the tail ions (CO^+ , CH^+ , N_2^+ , H_2O^+) near the nucleus. Observations of past comets indicate that CO^+ ions are present, at a distance of 10,000 km from the nucleus and with a density $\sim 10^3 \text{ cm}^{-3}$ (ref. 8). For some comets the observations suggest (though not unambiguously) that the tail ions originate still closer to the nucleus, where ion-molecular reaction and dissociative recombination should limit their lifetimes to a fraction of an hour only. For this reason spectrophotometric work (with adequate resolution to overcome the dust continuum) on the emission of ions close to the nucleus is envisaged.

The number and scope of the observing programmes planned for the coming months are very much larger than could be described in this short article, which represents only a very personal selection. For a much more complete survey of existing programmes reference is made to a report prepared on behalf of Commission 15 of the International Astronomical Union at Goddard Space Flight Center by J. C. Brandt, J. Rahe and V. Vanysek.

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Sea Sediments, Cosmic Rays, and Pulsars

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Measured variations in ^{10}Be and ^{26}Al in deep sea sediments may result from increases in the local cosmic ray intensities produced by a nearby supernova explosion.

MEASUREMENTS of time variations in the local cosmic ray intensity can provide significant information on the origin and propagation of cosmic rays. Long-lived radioisotopes produced by cosmic ray interactions in the Earth's atmosphere offer the best opportunity for studying the time dependence of the cosmic ray intensity¹.

The principal long-lived radioisotopes² produced by cosmic rays in the atmosphere are ^{10}Be and ^{26}Al with mean lives against decay of 2.3×10^6 yr and 1.1×10^6 yr respectively³. ^{10}Be is produced principally by spallation of nitrogen by cosmic ray protons, and ^{26}Al by spallation of argon. These isotopes are quickly precipitated from the atmosphere; however, their latitude dependent production rates are destroyed by ocean current mixing². They are deposited in deep sea sediments in times of the order of 10^3 yr at rates proportional to their global average production values⁴. The production rate of these isotopes as a function of time in the past can thus be related to their present activity as a function of depth:

$$q(t) \propto \tau s A(d) \exp(d/\tau s) \quad (1)$$

where $q(t)$ is the global average production rate (atoms $\text{s}^{-1} \text{cm}^{-2}$ of Earth's surface) at a time in the past, $t=d/s$; d is the depth (cm) in the sediment; s is the sedimentation rate (cm yr^{-1}); τ is the decay mean life (yr) of the radioisotope; and $A(d)$ is the specific activity of the radioisotope (decays $\text{s}^{-1} \text{cm}^{-3}$ of sediment).

Since the production rate $q(t)$ is proportional to the global average cosmic ray intensity, or energy density $w(t)$, then, assuming that the sedimentation rate is constant, the relative cosmic ray energy density may be written

$$w(t)/w(0) \approx \{A(d)/A(0)\} \exp(d/\tau s) \quad (2)$$

where $w(0)$ and $w(t)$ are the global-averaged cosmic ray energy densities at the present and at some time in the past $t=d/s$; $A(0)$ and $A(d)$ are the activities of the radioisotopes in sediment at depths 0 and d . From measurements of the activity of two radioisotopes of different decay mean lives, such as ^{10}Be and ^{26}Al , the sedimentation rate may be directly determined.

Sediment Rates of ^{10}Be and ^{26}Al

The measured activities of both isotopes in the segments of the two deep sea sediment cores are given in Table 1 (refs 5, 6). The sedimentation rate calculated for the ratio of ^{10}Be and ^{26}Al activities in the top and bottom segments of each core is found to be roughly $4 \times 10^{-5} \text{ cm yr}^{-1}$ with an uncertainty

of about $\pm 70\%$ because of the large uncertainty in the ^{26}Al activities. This value is consistent with independent estimates of the sedimentation rate. From the ratio of the specific activity of the ^{10}Be in the upper segments of the cores to the calculated production rate of ^{10}Be in the atmosphere, Yokoyama⁶ found a rate of $3 \times 10^{-5} \text{ cm yr}^{-1}$ with a probable uncertainty of $\pm 50\%$. From the ionium to thorium ratio, Goldberg and Koide⁷ found sediment rates ranging from 3 to $6 \times 10^{-5} \text{ cm yr}^{-1}$ for other South Pacific cores. Similar rates were determined from palaeomagnetic measurements by Ku *et al.*⁸. To study the possible range of the variations in the cosmic ray intensity, we consider two extreme values of the sedimentation rate, 3×10^{-5} and $6 \times 10^{-5} \text{ cm yr}^{-1}$.

Table 1 Activities of ^{10}Be and ^{26}Al in Segments of Two Deep Sea Sediment Cores

	(cm)	Activity (d.p.m. kg^{-1} dry sediment)	
		^{10}Be	^{26}Al
South Pacific core *	0-12	4.7 ± 0.5	0.78 ± 0.34
	12-27	4.6 ± 0.4	
	37.5-52.5	3.6 ± 0.5	
	52.5-67.5	3.0 ± 0.5	
	83-110	4.6 ± 0.5	
North Pacific core *	0-21.5	5.0 ± 0.4	0.9 ± 0.35
	100-120	2.1 ± 0.4	0.1 ± 0.5 -0.1

* The South Pacific core was obtained at $57^\circ 35' \text{ S}$, $174^\circ 15' \text{ W}$ at a depth of 4,760 m; the North Pacific core at $8^\circ 20' \text{ N}$, $145^\circ 24' \text{ W}$ at a depth of 5,110 m.

From equation (2), assuming a constant sedimentation rate, we can calculate the relative global-average ^{10}Be and ^{26}Al production rates as a function of time from the measured activities as a function of depth. The resultant values (Fig. 1) show rough constancy for the last 1 to 2×10^6 yr (depending on the sedimentation rate) preceded by a significant increase between ~ 1.4 and 3.5×10^6 yr ago. This increase is several standard deviations above the constant value and its magnitude ranges from roughly an 80% increase averaged over 0.5×10^6 yr to a 220% increase averaged over 10^6 yr. This increase cannot be due simply to mixing of recent material to depth because the measured ratio of $^{10}\text{Be}/^{26}\text{Al}$ activity differs significantly from that of recent sediment and in fact corresponds to that expected from sediment of between 1.5 and 4×10^6 yr age. Therefore, to account for the increased activity, we must look to either a decrease in the sedimentation rate or an increase in the global-averaged cosmic ray intensity.

If a decrease in the sedimentation rate were responsible for the increase in specific activity, the rate would have to have been less than 30% to 50% of the present rate. It cannot be due simply to removal of sediment. There is no evidence for such time variations in the sedimentation rate but the possibility cannot yet be excluded. Palaeomagnetic measurements of sediment cores made in conjunction with future ^{10}Be analyses will answer this question.

Cosmic Ray Intensity

If the increased activity is due to an increase in the global-averaged cosmic ray intensity, this could in principle be due either to a decrease in the Earth's magnetic field, which at present shields the atmosphere from a significant fraction of the cosmic rays at low latitudes, or to an increase in the local cosmic ray intensity. The possibility that it resulted from a reduction in the geomagnetic field can, however, be ruled out. Even during a field reversal when the field could go essentially to zero, the global averaged intensity² would only increase by about 90%. Since the measured increase in activity ranges from about 80% to 220% depending on the sediment rate, only in the lower limit could the increase possibly be due to a decrease in the magnetic field. Even then, however, the field would have to have remained essentially zero for 0.5×10^6 yr, which is contrary to the wealth of palaeomagnetic evidence⁹.

Thus, if the increase in activity is not caused by a decrease in the sedimentation rate, it must directly reflect an increase in the cosmic ray intensity. Such intensity increases could be expected from nearby supernova explosions if cosmic rays are produced either in the initial explosion or soon after as the pulsar rapidly slows down¹⁰. An estimate of the ages and distances of such supernovae can be made from pulsar observations, assuming that the pulsars are the neutron star remnants of supernovae. From these estimates we can attempt to correlate the apparent cosmic ray intensity increase with a particular supernova. Pulsar ages can be estimated from the measured period P and slowing down rate $\dot{P}$. General considerations¹¹ of the slowing down of neutron stars suggest that the age is between $P/2\dot{P}$ and $P/4\dot{P}$. Pulsar distances can be determined from the dispersion measure of their signals, assuming an interstellar electron density.

Of the 30 pulsars for which both P and $\dot{P}$ have been measured¹² roughly half have ages which lie between 1.4 and 3.5×10^6 yr. Of these, PSR1929 with an age of 1.6 to 3.3×10^6 yr is apparently the closest. It has a dispersion measure of only $3.2 \text{ cm}^{-3} \text{ pc}$. The next closest, PSR2045, has a dispersion measure of 11.5. The interstellar electron density ranges from about 0.1 cm^{-3} in ionised regions around pulsars

to 0.02 cm^{-3} in undisturbed regions¹³. Assuming a mean value of $\sim 0.05 \text{ cm}^{-3}$ gives a mean distance for PSR1929 of $\sim 60 \text{ pc}$ with an uncertainty of probably $\pm 30 \text{ pc}$. Since we are interested not in the present distance but in the distance some 1.6 to 3.3×10^6 yr ago, the unknown relative velocity of the pulsar with respect to the Sun adds an additional uncertainty in the distance at the time of the supernovae explosion when the cosmic rays were produced. Typical relative velocities of the order of $\sim 10 \text{ km s}^{-1}$ would lead to an uncertainty of about $\pm 15 \text{ pc}$ to $\pm 30 \text{ pc}$.

If we assume that the observed activity increase does reflect an increase in the local cosmic ray intensity caused by the supernova explosion of which PSR1929 is the remnant neutron star, we can then ask what cosmic ray total energy releases and what cosmic ray propagation processes are required to account for the magnitude and time dependence of the increase. Coupled with the two values of the sedimentation rate of 3×10^{-5} and $6 \times 10^{-5} \text{ cm yr}^{-1}$ we will consider the two limiting ages of the supernovae explosion of 3.3×10^6 and 1.6×10^6 yr, in both cases assuming a distance of 60 pc.

Cosmic Ray Propagation

Several processes of cosmic ray propagation have been considered¹⁴⁻¹⁶: simple streaming of cosmic rays in the interstellar medium; three-dimensional diffusion, either by scattering of cosmic rays from one magnetic field line to another or by streaming along random walking field lines; or compound diffusion, consisting of one-dimensional diffusion along field lines which random walk in three dimensions. Assuming a statistical distribution of discrete cosmic ray sources in space and time, Lingenfelter and Ramaty¹⁶ found that the measurements of anisotropy and abundance of secondary components (^2H , ^3He , LiBeB) required a mean free path of $\sim 0.1 \text{ pc}$ for cosmic ray propagation by three-dimensional diffusion and $\sim 30 \text{ pc}$ by compound diffusion.

We shall first consider, however, the possibility that the cosmic rays simply stream out radially from the supernova source. If the total cosmic ray energy produced in the source is W_{sn} , then the total cosmic ray increase at the Earth is $W_{\text{sn}}\pi r^2/4\pi d^2$ where r is the radius of the Earth and d is the distance of the source ($\sim 1.8 \times 10^{20} \text{ cm}$). This is equal to $4\pi r^2 \Delta w c T$ where Δ is the relative increase in measured activity, w is the present cosmic ray energy density ($10^{-12} \text{ erg cm}^{-3}$), c is the velocity of light and T is the time interval over which the increase Δ is observed. Thus we see that the required $W_{\text{sn}} = 16\pi d^2 c \Delta w T$. For a sedimentation rate of $6 \times 10^{-5} \text{ cm yr}^{-1}$, the increase $\Delta = 0.8$ and $T = 4.5 \times 10^5 \text{ yr}$, requiring a total cosmic ray energy production in the source $W_{\text{sn}} \approx 5 \times 10^{53} \text{ erg}$ to account for the measured increase in activity. For a rate of $3 \times 10^{-5} \text{ cm yr}^{-1}$, $\Delta = 2.2$ and $T = 9 \times 10^5$, requiring $W_{\text{sn}} \approx 3 \times 10^{54} \text{ erg}$. Both of the values are several orders of magnitude greater than the estimates of 10^{51-52} erg for the cosmic ray energy available in either supernovae explosions¹⁷ or in slowing down of neutron stars¹⁸. Thus, even allowing for the uncertainty in the distance, we conclude that cosmic ray propagation by streaming cannot account for the observed increase in activity.

Much smaller total cosmic ray energies are required, however, for cosmic ray propagation by diffusive processes. The time dependence of the increased cosmic ray energy density $\Delta w(t)$ at the Earth resulting from three-dimensional diffusion of cosmic rays from a nearby source of age t_0 and distance d is

$$\Delta w(t) = W_{\text{sn}} d^{-3} [2\pi(t_0 - t)/3t_m]^{-3/2} \exp\{-3t_m/2(t_0 - t)\}$$

where $t_m = d^2/2\lambda c$ and λ is the diffusion mean free path. Using a λ of 0.1 pc , we have calculated the time dependence $\Delta w(t)$ and normalised W_{sn} to give the observed relative increase in activity. This calculated increase added to a constant background is shown in Fig. 1 for the two sedimentation rates considered. For the rate of $6 \times 10^{-5} \text{ cm yr}^{-1}$ we use the minimum age of PSR1929, $t_0 = 1.6 \times 10^6 \text{ yr}$, and find that the

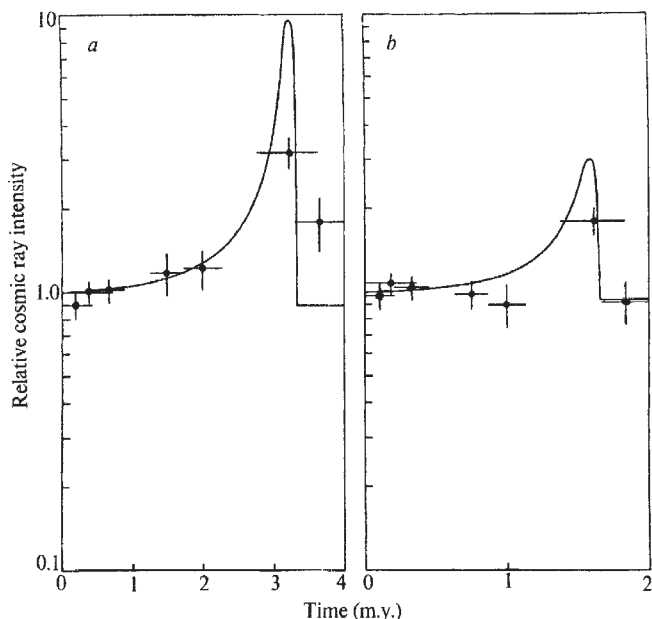


Fig. 1 Relative cosmic ray intensity as a function of time deduced from ^{10}Be activity in deep sea sediments, using two limiting cases for the sedimentation rate: a, $3 \times 10^{-5} \text{ cm yr}^{-1}$; b, $6 \times 10^{-5} \text{ cm yr}^{-1}$. The curve represents the intensity increase expected from the supernova explosion associated with pulsar PSR1929, assuming cosmic ray propagation by three-dimensional diffusion.

measured increase in activity requires a total cosmic ray energy in the source $W_{sn} \approx 1.8 \times 10^{50}$ erg. For the rate of 3×10^{-5} cm yr $^{-1}$ we use the maximum age of PSR1929, $t_0 = 3.3 \times 10^6$ yr, and find it requires $W_{sn} \approx 7.4 \times 10^{50}$ erg. These two values are in good agreement with the estimated cosmic ray energy available in such sources, mentioned above, and suggest that three-dimensional diffusion of cosmic rays from PSR1929 could indeed account for the observed increase in activity. This would require that at the present some 6% to 10% of the local cosmic ray intensity comes from this source. The associated anisotropy would be $< 4 \times 10^{-5}$ which is well below the present upper limit of $< 10^{-3}$.

Calculations of the increase in intensity expected from cosmic ray propagation by compound diffusion with a mean free path of ~ 30 pc give comparable values of W_{sn} and time dependences which are also consistent with the measured values.

There is reason, however, to question the applicability of both of these propagation models at early times near a source, since in both models it is assumed that the cosmic rays do not strongly affect the structure of the magnetic field through which they propagate. This in turn requires that the cosmic ray energy density does not greatly exceed the magnetic field energy density. Though this condition is apparently satisfied in much of the interstellar medium where the mean energy density of the cosmic rays and the magnetic field are approximately equal, it clearly would not be satisfied at early times near a source where the energy density of cosmic rays would necessarily be much greater than that of the ambient magnetic field. The early phase of cosmic ray propagation near a source has received relatively little attention, partly because of the complexity of the dynamical problems involved and partly because of the lack of suitable observations against which to test a model. If the correlation, suggested here, between the measured increase in activity and the supernova which produced

PSR1929 is significant, suitable observations are no longer lacking.

In view of the meagreness of the present data on variations of ^{10}Be and ^{26}Al activity in deep sea sediments this discussion has been highly speculative. But we hope that by pointing out the implications of the existing measurements we shall stimulate others to make further measurements which may either confirm or refute the variations in the cosmic ray intensity which have been suggested.

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Cyclic Nucleotides as Intracellular Mediators of the Expression of Antigen-sensitive Cells

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The interaction of antigen with antigen-sensitive cells can lead to antibody synthesis (induction) or can render these cells non-inducible (paralysis). Cyclic AMP may be the intracellular mediator of a paralytic signal whereas both cyclic AMP and cyclic GMP may be required as mediators of an inductive signal.

THE interaction of antigen with antigen-sensitive cells is required both for the induction of antibody synthesis (induction) and for rendering these cells non-inducible

(paralysis). We wish to discuss here the intracellular signals that may determine whether antigen-sensitive cells follow inductive or paralytic pathways. There is evidence that competition between these two pathways operates at the level of the antigen-sensitive cell itself¹. The delivery of two signals to antigen-sensitive cells leads to the induction of antibody synthesis: one of these signals is delivered through the antigen-binding receptor, the second through the cooperating cell system. In certain conditions bacterial lipopolysaccharides (LPS) can deliver the cooperating cell signal to antigen-sensitive cells²⁻⁴. The signal delivered to antigen-sensitive cells as a consequence of binding antigen appears to be common to both inductive and paralytic pathways. For example, deaggregated human gamma globulin is paralytic in adult mice but can be rendered immunogenic by LPS⁵. Similarly, DNP coupled to a non-immunogenic

polymer (D-glutamic acid-D-lysine) is itself paralytic but induces antibody synthesis when allogeneic lymphoid cells are used to provide the cooperating cell signal⁶. These experiments suggest that the interaction of antigen with receptors on antigen-sensitive cells leads to the paralytic pathway and that the cooperating cell signal diverts cells receiving the antigenic signal from a paralytic to an inductive pathway.

Many membrane-mediated events are coupled to an enzyme, adenylyl cyclase, which rapidly increases the intracellular concentration of adenosine 3',5'-cyclic phosphate (cyclic AMP)⁷. Several studies indicate that cyclic AMP directly affects the division of cells. High intracellular levels have an antimitotic effect on cells, and agents which stimulate confluent, normal cells to resume division decrease the intracellular levels of cyclic AMP⁸⁻¹¹. The addition of exogenous cyclic AMP to culture cells often results in the expression of a specialised function characteristic of the cell type. In neural tumours, cyclic AMP induces neurite formation¹²; in hepatomas, tyrosine transaminase^{13,14}; in melanomas, pigment production^{15,16}; and in lymphomas, cell death¹⁷. These findings suggest that cyclic AMP is an intracellular mediator of a programmed cell function and that increasing intracellular levels of cyclic AMP induces specialised cells to express their programme.

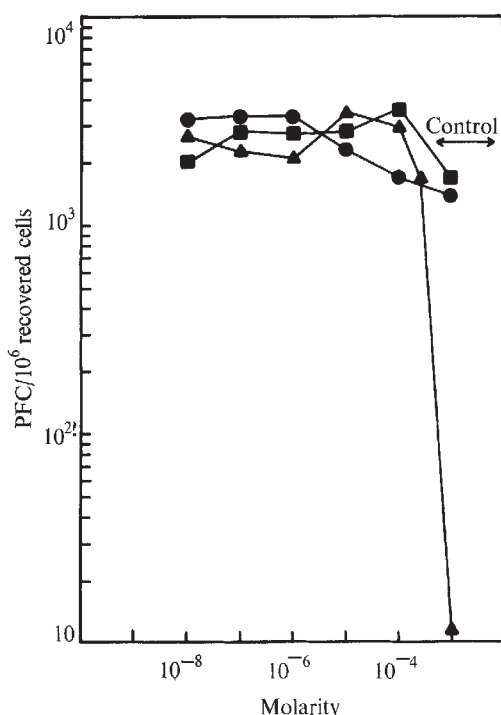


Fig. 1 Effect of different concentrations of dibutyryl cyclic AMP (▲), adenosine (AdR, ●) and adenylic acid (AMP, ■) on *in vitro* primary immune responses to SRBC. Spleen cultures were prepared from C57Bl/6 mice as described before^{2,3}. Each culture contained 1×10^7 spleen cells, 3×10^6 SRBC in 1 ml Eagle's medium, and assayed after 4 d in a microscope slide assay for haemolytic PFC. The control level denotes the PFC in culture lacking cyclic AMP, AdR or AMP. Adenosine N⁶, O^{2'}-dibutyryl cyclic AMP, 2', 3'- and 5' adenylic acids were obtained from Calbiochem.

Extracellular cyclic AMP has been reported both to stimulate¹⁸⁻²⁰ and to inhibit²¹ the induction of antibody synthesis. These studies do not distinguish between cyclic AMP having a general effect on the growth rates of induced antigen-sensitive cells, and cyclic AMP possessing a role in the signal system responsible for the induction process

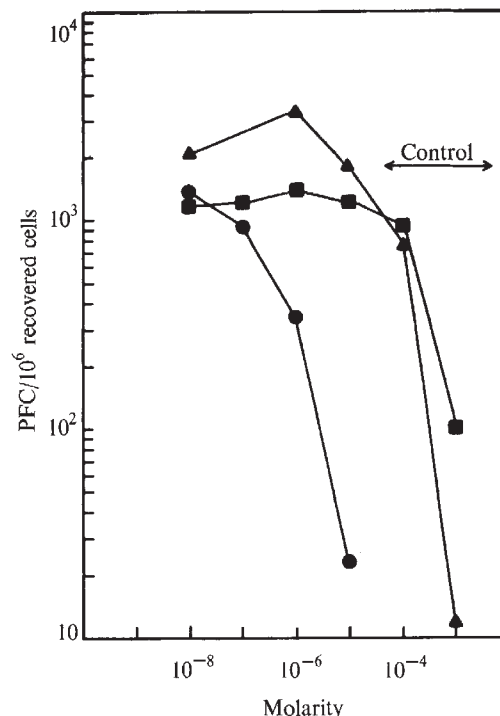


Fig. 2 Effect of theophylline (■) and papaverine (●) on the induction of *in vitro* primary immune responses to SRBC. Spleen cultures were prepared from C57Bl/6 mice and assayed on day 4. The control level denotes the immune response to SRBC in spleen cultures lacking papaverine or theophylline. Papaverine hydrochloride was obtained from S. B. Penick and Company, New York. Theophylline was obtained from Calbiochem. ▲, Dibutyryl cyclic AMP.

itself. We describe here the inactivation of the induction of an *in vitro* IgM response to sheep red blood cells (SRBC) by cyclic AMP and its reversal by guanosine 3',5'-cyclic phosphate (cyclic GMP). Further, in mouse spleen cultures depleted of cooperating activity that do not respond to SRBC, the addition of cyclic GMP permits induction. In view of these experiments as well as our knowledge of the ubiquitous role of cyclic nucleotides as intracellular mediators of programmed cell functions, we suggest that cyclic AMP and cyclic GMP mediate the intracellular signals which determine whether antigen-sensitive cells follow an inductive or a paralytic pathway. Cyclic AMP alone is postulated to mediate paralysis whereas both cyclic AMP and cyclic GMP are required for induction.

Cyclic Nucleotides and Induction

The effect of different concentrations of dibutyryl cyclic AMP on the induction of *in vitro* primary immune responses to sheep erythrocyte antigens (SRBC) in normal (C57Bl/6) spleen cultures is described in Fig. 1. Concentrations of dibutyryl cyclic AMP below 10^{-4} M had little effect on the *in vitro* immune responses to SRBC although some stimulation (two to five-fold) in the response was often observed in the presence of 10^{-5} M dibutyryl cyclic AMP. As the concentration of dibutyryl cyclic AMP was increased in spleen cultures from 10^{-4} M to 10^{-3} M, however, increasing inhibition of the immune response was observed. Both adenylic acid (AMP) and adenosine (AdR) when added to spleen cultures in the same concentration range had little effect on the induction of *in vitro* immune responses. The presence of 10^{-3} M AdR had a slight inhibitory effect (<20%) on the *in vitro* immune response in different experiments.

The experiments described in Fig. 2 compared the effects of theophylline and papaverine on the induction of *in vitro*

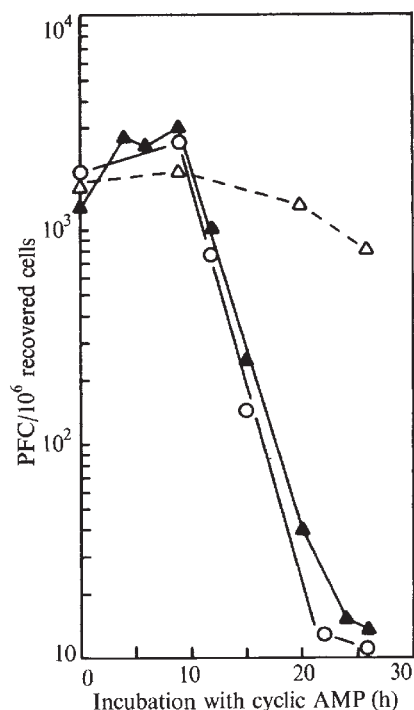


Fig. 3 Effect of time of incubation with cyclic AMP on the induction of *in vitro* primary immune responses to SRBC. Spleen cultures prepared from C57Bl/6 mice were incubated with 10^{-3} M dibutyryl cyclic AMP in the presence ($\blacktriangle$) and absence ($\circ$) of SRBC for the times indicated. Cultures were then washed and resuspended in fresh Eagle's medium (1 ml). Cultures lacking SRBC were then supplemented with 3×10^6 SRBC. The control cultures ($\triangle$) were spleen cells incubated in the absence of SRBC and dibutyryl cyclic AMP, and at the times indicated washed and recultured in fresh Eagle's medium with 3×10^6 SRBC. All spleen cells were assayed for SRBC-specific PFC 4 d after the start of cultures. For removal of cyclic AMP from cultures, medium containing non-adherent cells was removed gently from each spleen culture and each dish was washed once by adding back and removing a 1 ml sample of fresh medium. The supernatant collected from each culture dish was centrifuged at 850g for 5 min, removed and each cell pellet was resuspended in 1 ml medium and recultured in the dish from which it was originally collected.

immune responses to SRBC. While theophylline at 10^{-3} M inhibited the immune response about ten-fold, papaverine at 10^{-5} M suppressed induction 100-fold (Fig. 2). Since both theophylline and papaverine inhibit phosphodiesterase activity and lead to the accumulation of intracellular cyclic AMP^{22,23}, the inhibition of immune responses observed in these experiments seems to be mediated intracellularly by cyclic AMP.

The kinetics of the establishment of *in vitro* immune unresponsiveness by pulses of 10^{-3} M dibutyryl cyclic AMP is described in Fig. 3. When the extracellular dibutyryl cyclic AMP was removed at various times after the start of spleen cultures, two effects were observed. First, when removed up to 9 h after addition to spleen cultures, there was characteristically a stimulation of the subsequent immune response to SRBC. This has varied in our experiments from two to six-fold. Second, between 12 and 20 h after addition of dibutyryl cyclic AMP to spleen cultures there occurred an exponential decrease in the subsequent immune response to SRBC. The half-life of the ability to respond to SRBC during this decline was about 3 h. The presence or absence of SRBC during exposure of spleen cells to 10^{-3} M dibutyryl cyclic AMP did not affect the inhibition of immune responsiveness to SRBC (Fig. 3), this possibly being due to the concentration of cyclic AMP used.

The induction of *in vitro* immune responses to SRBC requires collaboration between two types of antigen-sensitive

cells, the bone marrow-derived precursor cell (P-AFC) and the thymus-derived (T) cells. If dibutyryl cyclic AMP inhibited the expression of either or both of these lymphoid cell types the *in vitro* immune responses would be suppressed. Consequently, we have examined the effects of dibutyryl cyclic AMP on the expression of P-AFC and T cells in two ways.

First, a previous finding was utilised to examine the effect of dibutyryl cyclic AMP on P-AFC^{2,3}. Bacterial lipopolysaccharides (LPS) permit a response to antigen in spleen cultures depleted of T cells. Congenitally athymic (nude) mice are deficient in T cells^{2,3}. Nude spleen cultures containing SRBC were exposed to 10^{-3} M dibutyryl cyclic AMP for various times, the cyclic AMP then removed and 5 μ g LPS added to the cultures. After 4 d, cultures were analysed for their immune response to SRBC. The results are described in Fig. 4A. When compared with control cultures (nude spleen cells not exposed to dibutyryl cyclic AMP) it can be seen that increasing the length of time

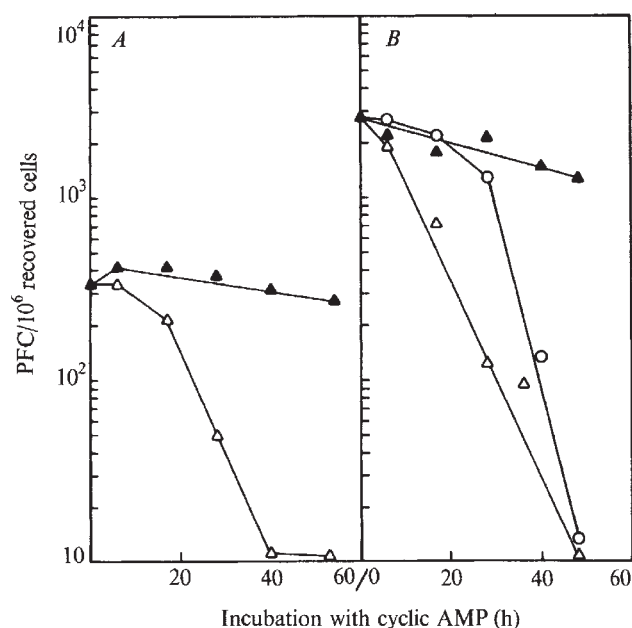


Fig. 4 The kinetics of inactivation of P-AFC and T_{SRBC} cells by 10^{-3} M dibutyryl cyclic AMP. **A**, Control cultures ($\blacktriangle$) containing 10^7 nude spleen cells were incubated, washed and recultured with 5 μ g LPS and 3×10^6 SRBC ($t_i=40$ h); + cyclic AMP ($\triangle$) cultures containing 10^7 nude spleen cells were incubated with 10^{-3} M dibutyryl cyclic AMP for the times indicated, washed and recultured with 5 μ g LPS and 3×10^6 SRBC ($t_i=5$ h). **B**, Control cultures ($\blacktriangle$) containing 10^7 nude spleen cells or 5×10^6 T_{SRBC} cells were incubated without cyclic AMP and washed at the times indicated. The cells from one nude culture were mixed with those from one T_{SRBC} culture, 5×10^6 SRBC added and then incubated another 4 d before assaying ($t_i=38$ h); P-AFC cultures ($\triangle$) containing 10^7 nude spleen cells were incubated with 10^{-3} M dibutyryl cyclic AMP for the times indicated, washed and then recultured with 5×10^6 T_{SRBC} cells which had not been treated with cyclic AMP and 3×10^6 SRBC ($t_i=7$ h); T_{SRBC} cultures ($\circ$) containing 5×10^6 T_{SRBC} cells were incubated with 10^{-3} M dibutyryl cyclic AMP for the times indicated, washed and then recultured with 10^7 nude spleen cells which had not been treated with cyclic AMP, and 3×10^6 SRBC ($t_i=3$ h). Mice homozygous for the mutation nude were bred as described elsewhere³. *Salmonella typhosa* lipopolysaccharide (LPS) (Difco) was dissolved in 0.1 M phosphate buffer (pH 8.0) at a concentration of 1 mg ml⁻¹ and boiled for 60 min before use. To prepare T_{SRBC} , C57Bl/6 mice were irradiated (850 r) using cobalt-60 and then injected intraperitoneally with 100 U heparin (Upjohn). After 10 min these irradiated mice were injected intravenously with 0.2 ml of a solution containing $5-10 \times 10^7$ thymus cells from non-irradiated C57Bl/6 donors and 2×10^8 SRBC. After 5 to 8 d the spleen cells were isolated from the irradiated mice and used as the source of activated thymus cells.

of exposure to dibutyryl cyclic AMP decreases the ability of LPS to stimulate SRBC responses in nude cultures (Fig. 4A). From these data the following calculations can be made. The P-AFC in control nude spleen cultures has a background half-life of 40 h as determined by ability to respond when challenged with 5 μ g LPS and SRBC. In contrast, in nude spleen cultures pulsed with 10^{-3} M dibutyryl cyclic AMP, and then challenged with 5 μ g LPS and SRBC, the half-life of the P-AFC is decreased to about 5 h.

Second, these results were extended by comparing the inactivation of P-AFC by dibutyryl cyclic AMP using T cells activated to SRBC (T_{SRBC}) in place of LPS. The rate of inactivation of P-AFC was determined by treating nude spleen cultures with 10^{-3} M dibutyryl cyclic AMP for varying times and assaying the remaining immune response to SRBC by the addition of SRBC and an excess of T_{SRBC} cells. Control nude spleen cultures were treated in exactly the same way except that dibutyryl cyclic AMP was not added to the nude spleen cultures. All cultures were assayed for immune responses to SRBC 4 d after the end of the cyclic AMP pulse period and the addition of T_{SRBC} and SRBC. These data have been plotted in Fig. 4B. The results are similar to those obtained using LPS (Fig. 4A). The P-AFC in control nude spleen cultures had a half-life of 38 h, and in dibutyryl cyclic AMP-treated cultures a half-life of about 7 h.

Table 1 Effect of Cyclic GMP on Induction of Immune Responses to SRBC in Normal (C57Bl/6) and T Cell-depleted (Nude) Spleen Cultures

Concentration of cyclic GMP (M)*	PFC/ 10^6 recovered cells (day 4)		
	C57Bl/6 †	Nude ‡	Nude ‡
None added	568	15	21
5.0×10^{-3}	310	116	29
2.5×10^{-3}	376	89	458
1.0×10^{-3}	413	57	496
5.0×10^{-4}	976	29	348
2.5×10^{-4}	571	12	241
1.0×10^{-4}	891	<10	169
1.0×10^{-5}	763	<10	36
1.0×10^{-6}	516	<10	<10
1.0×10^{-7}	617	<10	<10

The results represent the mean of duplicate cultures and are representative of at least four separate experiments. Culture procedures are described in the legend to Fig. 1. Cyclic GMP was obtained from Calbiochem.

* Cyclic GMP added at start of cultures.

† Cultures not fed with cyclic GMP.

‡ Cultures fed daily with 5.0×10^{-4} M cyclic GMP.

The effect of dibutyryl cyclic AMP on the expression of activity in the T_{SRBC} cell population was also determined. Here, cultures containing 5×10^6 T_{SRBC} cells were exposed to 10^{-3} M dibutyryl cyclic AMP for various times, removed and 10^7 untreated nude spleen cells and SRBC added to each culture. Again, 4 d after the pulse with dibutyryl cyclic AMP cultures were assayed for immune responses against SRBC. When compared with the control cultures (Fig. 4B), it was apparent that dibutyryl cyclic AMP also affected the activity of T_{SRBC} cells. Since in nude spleen cultures containing fresh T_{SRBC} cells, it is the T cells which are in excess, the half-life of T_{SRBC} cells can be determined only when their activity becomes limiting. Thus, T_{SRBC} cells can be assumed to be limiting when the PFC response decreases exponentially. These data indicate that the half-life of T_{SRBC} in the presence of dibutyryl cyclic AMP is about 3 h.

The addition of cyclic GMP to normal (C57Bl/6) spleen cultures in the range of 10^{-3} M to 10^{-8} M had little effect

on the induction of immune responses to SRBC (Table 1). The addition of cyclic GMP to nude spleen cultures containing SRBC resulted, however, in the stimulation of immune responses to SRBC (Table 1). Feeding the cultures with cyclic GMP each day seems to enhance the *in vitro* immune responses to SRBC. Since nude spleen cells are deficient in the cooperating T cell population it seems that the cyclic GMP may be replacing the function of T cells.

Table 2 Effect of Cyclic GMP on Establishing Immune Unresponsiveness by Cyclic AMP

Cyclic nucleotides in cultures *		PFC/ 10^6 recovered cells (day 4) †
Cyclic AMP (M)	Cyclic GMP (M)	
None added	None added	848
10^{-3}	None added	26
10^{-3}	10^{-3}	21
10^{-3}	10^{-4}	525
10^{-3}	10^{-5}	268
10^{-3}	10^{-6}	114

* Cyclic GMP and cyclic AMP were present in spleen cultures prepared from C57Bl/6 mice for the initial 24 h, then cells were washed, recultured in fresh medium and left a further 3 d before assaying.

† Figures represent the mean of triplicate cultures and are representative of three separate experiments. Culture procedures are identical to those described in the legends to Figs 3 and 4.

Treatment of spleen cultures with 10^{-3} M dibutyryl cyclic AMP for 24 h was sufficient to induce immune unresponsiveness. When spleen cultures containing 10^{-3} M dibutyryl cyclic AMP and different concentrations of cyclic GMP (10^{-3} M to 10^{-5} M) were washed and recultured after exposure for 24 hours to these cyclic nucleotides, two effects were observed (Table 2). First, cyclic GMP reverses the inactivating effects of cyclic AMP. Second, there is an optimum ratio of cyclic AMP/cyclic GMP for this reversal. Finally, we should mention that cyclic GMP seems to be better than dibutyryl cyclic GMP in stimulating these various responses (unpublished data). All data reported here have been obtained using cyclic GMP.

Cyclic Nucleotides: Intracellular Mediators

The small number of antigen-sensitive cells in the spleen that respond to any given antigen makes it difficult to analyse the biochemical events associated with the interaction of antigen with cell surface receptors. High concentrations of cyclic AMP inhibit the induction of *in vitro* immune responses; this inhibition is reversed by the presence of cyclic GMP. In the absence of T cells, cyclic GMP induces immune responses. We have based our interpretation of these data on three points: (a) each antigen-sensitive cell can be induced or paralysed¹; (b) the interaction of antigen with the receptor on antigen-sensitive cells leads to the paralytic pathway^{5,6}; (c) induction requires in addition to the signal initiated by the antigen-receptor interaction a second signal initiated by the thymus-derived cooperating system^{3,5,6}.

We suggest that the binding of antigen to receptors on antigen-sensitive cells activates the adenylyl cyclase system to increase intracellular levels of cyclic AMP. The increase in cyclic AMP triggers the paralytic pathway. The evidence for this is the finding that high concentrations of cyclic AMP inactivate both P-AFC and T_{SRBC} cells (Fig. 4). A comparison by cyclic AMP inactivation of antigen-sensitive cells, both P-AFC and T cells (Fig. 4), with their inactivation (paralysis) by antigen²⁴, shows several similarities. Although both processes render cells non-inducible, there is a different rate of inactivation for each antigen-sensitive cell type.

At saturating concentrations of dibutyryl cyclic AMP (10^{-3} M) the half-life of the bone-marrow-derived P-AFC is approximately 6 h while that of the cooperating thymus-derived T cell is around 3 h (Fig. 4). In normal spleen cultures, cyclic AMP inactivates the response to SRBC with a half-life of 3 h, showing, as expected, that it is the T cell which first becomes limiting (Fig. 3). These *in vitro* data are in accord with *in vivo* data showing a greater rate of paralysis by antigen of thymus-derived cells as compared with bone-marrow-derived cells²⁴. Further evidence that cyclic AMP is the intracellular mediator of the inactivation process comes from the fact that phosphodiesterase inhibitors, theophylline and papaverine also render cultures unresponsive (Fig. 2). Since similar concentrations of cyclic AMP do not kill other cell types but rather induce such cells to express a differentiated function¹²⁻¹⁷ we believe the cyclic AMP inactivation of antigen-sensitive cells is revealing a differentiated pathway unique to these cells. The ability of cyclic GMP to reverse the cyclic AMP inactivation of antigen-sensitive cells supports this contention (see below).

We suggest that the second signal delivered by the interaction of the cooperating cell system with antigen-sensitive cells activates a guanyl cyclase system to increase intracellular cyclic GMP. Induction requires activation of both adenyl (signal (1)) and guanyl (signal (2)) cyclase systems. The evidence for this is that spleens of nude mice deficient in the thymus-derived cooperating system will respond to SRBC if cyclic GMP is added to the system. Under these conditions signal (1) comes to the P-AFC from the SRBC-receptor interaction and signal (2) from cyclic GMP. The membrane interaction which leads to an increase in the intracellular cyclic GMP level in the P-AFC normally requires a component from the cooperating cell. As far as the P-AFC is concerned, increasing cyclic GMP levels is the first intracellular component of the series of events we call signal (2).

Since the interaction of antigen with antigen-sensitive cells seems to deliver a signal common to both inductive and paralytic pathways, it is a requirement of the above two proposals that the inactivation by cyclic AMP be reversed by cyclic GMP, and this is found (Table 2). Simply reversing cyclic AMP inactivation is insufficient to explain induction because the antigen-sensitive cell would be left essentially in the state it was before encountering antigen. Therefore, cyclic GMP must have a further role. Induction must depend on the simultaneous presence at an effective level of both cyclic AMP and cyclic GMP. In other words, the cell must be able to read the ratios of cyclic AMP to cyclic GMP. It is important to note that there is an optimal ratio for reversal of cyclic AMP inactivation. At a given level of cyclic AMP, concentrations of cyclic GMP higher than optimum lead to a suppression of the response (Table 3). This fact taken as biologically significant implies that when the cooperating activity rises above a given level due to induction, P-AFC programmes for IgM secretion are switched off.

There are agents which interact with lymphocytes to induce proliferation. In the case of the phytohaemagglutinin or concanavalin A-induced proliferation of thymus-derived lymphocytes, optimum mitogenic concentrations of these substances produce rapid ten- to fifty-fold increases in intracellular cyclic GMP while little change occurs in cyclic AMP²⁵. In the case of LPS, its effect on the cyclic GMP level in bone marrow-derived lymphocytes has not yet been measured. These mitogens also affect the immune response so as to either enhance or inhibit. Under the present formulation it is to be expected that agents which affect cyclic GMP levels in the cell would also affect proliferation and induction both positively and negatively.

The closing comment involves the regulation of the cyclic AMP/cyclic GMP ratio by a balance between the rates of their synthesis and degradation by phosphodiesterases.

It is known that the two cyclic AMP phosphodiesterases from rat thymic lymphocytes exhibit different K_m values, 0.9×10^{-6} M and 8.0×10^{-6} M²⁶. The enzyme with the higher K_m is stimulated by cyclic GMP by increasing the V_{max} with no change in the K_m for cyclic AMP. Cyclic GMP competitively inhibits the enzyme with the lower K_m ²⁷. A partially purified preparation of phosphodiesterase from bovine heart had an apparent K_m value of around 2.0×10^{-6} M for cyclic GMP and more than 25×10^{-6} M for cyclic AMP. Both cyclic AMP and cyclic GMP interfered with the hydrolysis of each other in a manner predictable on the basis of their apparent K_m values. In liver extracts, cyclic GMP was found to stimulate hydrolysis of cyclic AMP²⁷. The finding that one cyclic nucleotide can alter the rate of hydrolysis of the other raises the possibility that the intracellular concentration of one cyclic nucleotide regulates the level of the other by influencing its rate of degradation. These studies provide a rationale as to why the two intracellular mediators of the inductive signal might be structurally related. Fine control of the ratio between them is possible, such that in one range it is buffered, and in another range it has a sharp rate of change. These considerations deal with the regulation of the ratio cyclic AMP/cyclic GMP but they leave open two questions. First, how is the ratio sensed by the antigen-sensitive cell? Second, in addition to reversing cyclic AMP inactivation, what is the pathway unique to induction that cyclic GMP triggers?

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LETTERS TO NATURE

PHYSICAL SCIENCES

Nature and Origin of Comets

IN so many fields of astrophysics the textbooks and the ordinary review articles have a tendency to present only the 'generally accepted' point of view, often not mentioning alternative theories, and even sweeping under the rug those observational facts which are adverse to the dominant view. This also applies to the theory of comets. As the arrival of Comet Kohoutek, and also the space missions to comets which are now being planned, is focusing the interest on comets, we may have unique opportunities in the near future to clarify which views are correct. This calls for a survey of the controversial points. A partial list of these, with which we will be concerning ourselves here, is the following:

- (1) What is the physical nature of the cometary nucleus?
- (a) Does it invariably consist of a single monolith? or (b) could it, in some instances at least, constitute a bunch of particles having no physical coherence?
- (2) What is the genetic relationship between comets and the meteor streams with which they are associated? (a) Do the stream meteoroids invariably derive from the associated comets? or (b) is the reverse process also possible?
- (3) Is there a net dissipation or accretion during the lifetime of a comet?
- (4) What is the origin of short-period comets?
- (5) What is the origin of long-period comets?

Several other questions with regard to the chemical composition of the nucleus, the mechanisms for the production of the observed radicals and ions, and the nature of the comet solar-wind interaction merit discussion but are outside the scope of this letter.

The answers to all these questions are basically connected with the problem of how a swarm of particles in similar orbits will develop. There is a general belief that the collisions (and other types of interaction) between the particles will cause a diffusion so that a state according to Fig. 1a will develop to the state depicted in Fig. 1b. This is correct only under the assumption that the collisions are elastic (or have at least a minimum of elasticity). But a swarm of particles in periodic orbits whose collisions are inelastic will behave in the contrary way, passing from the state of Fig. 1b to the state of Fig. 1a. This is because inelastic collisions cause the orbital parameters to become more similar. Baxter and Thompson^{1,2} have treated the problem with the Boltzmann formalism and demonstrated that the 'diffusion coefficient' is negative. The same result may be produced if the particles in the swarm exchange momenta in other ways, such as by the particles emitting gas which in turn interact with other particles.

As we do not know the collisional properties of the meteoroids in space, it is impossible to decide whether the diffusion in a stream of particles is positive or negative. This cannot be clarified by the study of meteorites which have fallen on the Earth because their surface properties, which decide their collisional behaviour, have been drastically changed during their passage through the atmosphere. Furthermore these meteorites are a highly selected sample because only those meteoroids having high tensile strength and consequently high elasticity can survive the passage through the Earth's

atmosphere. Meteorites collected on the Moon's surface have impinged with at least the lunar escape velocity and have also been changed. From a study of them, Arrhenius and Asunma¹⁹ have put forward strong arguments for the view that space collisions are inelastic, so that the diffusion should be negative. Also luminosity and deceleration studies of stream meteoroids in the Earth's atmosphere lead to the conclusion that most of these have bulk densities well under 0.5 g cm^{-2} whereas in the case of the Giacobinids they may be as low as 0.01 g cm^{-3} (ref. 3), suggesting also low elasticities.

The first alternative of positive diffusion leads necessarily to the more generally accepted theory of the comet-meteor stream complex⁴. In this case the stream meteoroids which derive from a big solid chunk of ice and dust (Whipple's 'icy conglomerate') must diffuse both along and normal to the stream to be ultimately dispersed into interplanetary space (sporadic meteors).

According to the second alternative of negative diffusion a meteor stream can be kept together under the condition that the self-focusing effect exceeds the disruptive effects due to planetary perturbations and solar radiation (Poynting-Robertson effect and so on). As shown by Mendis⁵ this seems likely to occur under very general conditions. So a meteor stream may behave as a typical 'jet stream' discussed by Alfvén and Arrhenius⁶. Due to its very large accretion cross-section a meteor stream may also be able to collect a significant amount of interplanetary dust and gas⁵.

Further, Trulsen⁷ has shown that planetary perturbations of meteor streams, rather than producing a general disruption, may cause density waves which build up slowly. If several

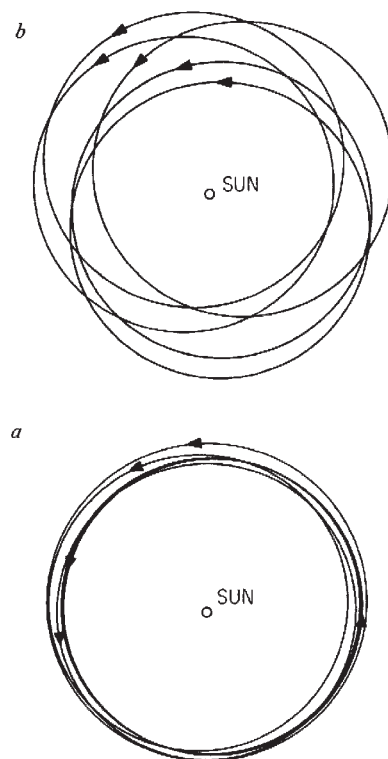


Fig. 1

such waves are progressing, very large density increases can be caused statistically at some points, leading to the formation of a dense cloud of particles. These in turn would ultimately agglomerate into a number of large chunks which may coalesce into one. This view then leads not to a single model of the cometary nucleus (as Whipple's 'icy conglomerate' or Lyttleton's 'sand bank') but rather is a whole hierarchy of models ranging all the way from a more or less loose agglomeration of small particles to a single monolithic nucleus, with the latter the most likely final state. Therefore although most comets possibly do have a single monolithic central nucleus, perhaps of the Whipple type, the possibility that there are comets with more than one nucleus or even consisting of a more or less loose swarm of grains cannot be ruled out. Indeed, the very dusty, gas deficient comets may belong to the latter type. There are several instances of observation of comets with multiple nuclei⁸⁻¹⁰. Whether these are the remnants of a single monolithic nucleus or merely the precursors of one is still an open question.

As regards the formation of comets in meteor streams there are several interesting observations which are suggestive of the latter process. For instance comet P/Temple-Tuttle ($P \approx 33.2$ yr) was first recorded as a diffuse but bright object only as recently as 1866 (ref. 11), although the associated Leonid meteor stream was known for centuries earlier. An even more significant observation concerns comet P/Swift-Tuttle ($P \approx 120$ yr) which was bright enough on its first apparition (in 1962) to be easily seen with the naked eye, being a 2 mag object at its brightest¹¹. What is surprising is its association with the Perseids meteor stream which has been observed for over twelve centuries¹².

There have been several claims by observers of actually having witnessed the formation of cometary nuclei⁹, but one hesitates to take these early observations at face value. More recently, however, Mrkos¹⁰ has stated that in the case of P/Honda-Mrkos-Pajdusakova no nucleus is seen when the heliocentric distance of the comet is > 0.7 AU even if it happens to be very close to the Earth (< 0.3 AU), while later on one begins to see not just one centre of light but several. Besides, this behaviour has been observed repeatedly. Interestingly, the comet probably also has an associated meteor stream (α -Capricornids).

We now turn to the question of the origin of long-period and short-period comets. The origin of the long-period comets is essentially unknown. The view that seems to be most generally held at the present time is that the long-period comets derived from the swarm of planetesimals out of which the outer 'icy' planets Uranus and Neptune (and perhaps to a lesser extent Saturn) formed. A small fraction of these planetesimals are believed to have been perturbed by the large planets into highly elongated orbits to form the long-period comets¹³. This view is basically not inconsistent with the planetesimal view of the formation of the Solar System (see, for example, Alfvén and Arrhenius (refs 6 and 14, and unpublished work), but long-period comets may still be forming as condensations in long-period meteor streams just as the short-period comets seem to do⁵.

As for the origin of short-period comets, the dominant view is that short-period comets derive from long-period comets that pass near one of the massive planets (especially Jupiter) and lose energy in the process. While single close encounters with Jupiter of the observed distribution of long-period comets are shown to be incapable of producing the observed distribution of short-period comets^{15,16}, Everhart¹⁷ has recently shown that the cumulative effect of many hundreds of passages near Jupiter of a low inclination belt of near-parabolic comets having initial perihelia near Jupiter's orbit could in fact produce such a distribution. It is, however, not clear at this stage if Everhart's calculations can resolve the crucial problem with regard to the origin of short-period comets, viz. the large observed number of these objects. P. C. Joss (unpublished) has shown on the basis of Oort's comet cloud

and the injection rate of new comets from this cloud into the inner Solar System that the above calculations fail by several orders of magnitude to explain the observed number of short-period comets. One can get around this difficulty, but only at the expense of introducing a new hypothesis, such as the existence of another distribution of long-period comets besides the observed one. This would be a disk distribution close to the ecliptic having dimensions $\lesssim 10^4$ AU and containing more than 10^9 comets⁵.

Everhart¹⁸ has recently shown that the observed distribution of short-period comets may also be derived by a process of diffusion from low inclination comet belts lying in between the major outer planets. There is, however, no evidence at the present time for any such belts.

But if one accepts that comets can be formed in meteor streams at the present time despite the competing disruptive processes, as has been shown by Trulsen⁷ and Mendis⁵, then this crucial difficulty with regard to the observed number of short-period comets is overcome. Meteor streams according to this view do not merely form a sink for short-period comets as is generally believed but also form a source, and a steady state may be maintained with the average rate of formation equalling the average rate of disruption⁵.

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Visual Determination of the Mean Effective Chromospheric Thickness

IN the course of taking submillimetre chromospheric data from Concorde 001 during the eclipse of June 30 (ref. 1), it proved possible to record information on the visual thickness of the chromosphere which is accurate enough to merit reporting. During an eclipse viewed conventionally from a stationary site on the ground, the precise time at which the solar photosphere reappears after totality, termed third contact, may be predicted to within 0.5 s (of time), given an adequate knowledge of the lunar limb topography, that is, to within 1 km in height at the appropriate point on the limb. Further, photometric measurements in white light could determine this time to within 0.3 s. It may also be possible

from the ground, under ideal observing conditions, to see and measure emission from the chromosphere which should be visible in isolation for around 3 s, just prior to third contact. The predominant radiation seen during this time is that of the first line of the Balmer series, $H\alpha$, at 6,563 Å which accounts for the traditional description of the phenomenon as the 'red flash'. It should be possible, after taking into account atmospheric scattering and diffraction effects, to measure the duration of this to within 0.5 s, though measurements of this type are not typically considered sufficiently accurate to be worth taking, when, for example, the flash spectrum of the chromosphere is also available². Although various observers^{3,4} have studied the chromosphere spectrally in emission during eclipse, both just after second contact and just before third contact, inaccuracies in our knowledge of the precise orientation of the Moon and its topography have made absolute heights above the photosphere very difficult to measure to better than 300 km, although relative heights may be estimated to within 100 km under ideal seeing conditions. Similar restrictions due to seeing apply to estimates based on $H\alpha$ filtergrams taken outside eclipse conditions⁵.

The apparent retardation of the eclipse by a factor of 15 when viewed from the moving aircraft provided a straightforward method of estimating the effective chromospheric thickness. With the eye entirely dark adapted after 74 min of totality, one could observe quite precisely a chromospheric third contact, followed 50 s later by photospheric third contact. As chromospheric third contact (CTC) occurred, the eye detected a sudden brightening at a point on the lunar limb which grew into a vivid red crescent. Through the 1-inch objective guider telescope for the main infrared experiment the crescent was seen to grow until it subtended an angle of some 40° at the disk centre, when a second step up in brightness occurred, and the colour changed at one point on the rim to the yellow of photospheric emission, thus marking photospheric third contact (PTC). This point too grew rapidly into a crescent, signalling that the aircraft had been overtaken by the edge of the umbra of the eclipse.

A fairly accurate estimate of the time interval between CTC and PTC was fortunately possible, as the observer was noting all observational changes verbally on a tape recorder. Subsequent timing showed that the estimate was subject to an error of up to 2.5 s at CTC and 0.5 s at PTC, the former error being due to a brief delay in recognising the phenomenon in unique circumstances. It is thus possible to state that the $H\alpha$ emission from the chromosphere was seen in isolation for 50 ($^{+3.0}_{-1.0}$) s. The precise position of the plane for every second of time to within 1 km in terrestrial latitude and longitude, and the speed to 1 part in 10⁴ were provided by the navigator, M. Henri Perrier, using data from the inertia navigation system. With the aid of these data, and with the aid of subsequent computation by HM Nautical Almanac Office of the positions and standard semi-diameters of Sun and Moon seen from each point on the aircraft's trajectory, the observed time interval is convertible to an angular and then linear thickness of observed chromosphere. The apparent rate of passage of the Moon's limb across that of the Sun at both third contacts was 0.0795 arc s s⁻¹, while the linear scale for the Sun's disk was 1 arc s = 737.4 km. These conversion factors combine to yield a value for the chromospheric $H\alpha$ thickness of 2,900 ($^{+200}_{-70}$) km above the photosphere at the time of the observation.

It is clear that this estimate does not imply any particular model for the chromosphere. No attempt could be made to deduce whether the radiation seen originates from a spherically symmetrical stratified layer or from inhomogeneities, in particular from spicules, known to be strong $H\alpha$ emitters, which cluster favourably in regions of enhanced magnetic fields along supergranular boundaries within the chromosphere⁶. There can be little doubt, however, in the light of other observations^{7,8} that spicules, which are optically thick in $H\alpha$ and the longest of which

extend in length to well over 10,000 km above the photosphere, could provide the bulk of the observed emission when viewed edge on in this way. It is estimated by Athay⁹ that, given the measured dimensions and properties of individual spicules as seen on the disk, their appearance seen edge on in $H\alpha$ light at eclipse would cease to be continuous at the height where they occupy 1% of the chromosphere surface area, above which individual spicules could be photographed separately. The present observations would set this 1% height close to 3,000 km above the photosphere. Further information on the distribution of spicules with height should become available when the sub-millimetre data, which were the ostensible objective of the experiment, have been fully reduced.

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Search for Atmospheric Gravity Waves induced by the Eclipse of June 30, 1973

Chimonas and Hines^{1,2} suggested that atmospheric gravity waves could be generated by the supersonic passage of the Moon's shadow through the Earth's atmosphere during a solar eclipse. Beer and May³ pointed out that the eclipse of June 30, 1973, would provide an opportunity of investigating the idea, and perhaps establish a link between travelling ionospheric disturbances (TIDs), as reported by other observers^{4,5}, and the eclipse phenomena. In addition, Beer and May predicted the positions of the hypothetical wavefronts for various times during and after the eclipse.

The gravity waves were predicted to have a period of 4 h and amplitude 10 μ bar at a position 10,000 km from the eclipse path¹. The TIDs reported around the times of previous eclipses had periods of the order of 20 min (refs 4 and 5). In an attempt to discriminate between the longer and shorter periods, a microbarograph was constructed with a variable pass band similar to that designed by Jones and Forbes⁶.

Our version of this instrument has a pass band determined

by two needle valves, which control slow and fast leaks to two otherwise identical metal bellows. The response function of this arrangement to sinusoidal variations in pressure is deducible theoretically, and for a typical configuration is shown in Fig. 1a. Pressure changes are measured by a plate which, attached to one of the bellows, forms part of a variable capacitor in a bridge network.

For calibration the whole instrument was placed in a sealed insulated pressure vessel, and the time constants τ_1 of the needle valves determined by maintaining one in a measured, almost closed position, while subjecting the instrument to a step-function pressure change; the measurement could then be repeated for a new set position. Figure 1b shows typical exponential outputs from which τ_1 are deduced, and Fig. 1c gives the experimental responses to sinusoidal pressure oscillations of different frequencies. Finally, an absolute amplitude calibration at zero frequency (that is, a static pressure response) was found.

On June 30 the instrument was set with a pass band of 30 to 7,000 s, giving a response of 145 μ bar full scale deflection for waves with periods of 20 min. With such a low frequency cutoff, the instrument required fairly frequent readjustment to bring it back on scale. The trace for the period in which the gravity waves might have been expected was very 'noisy' due to the weather conditions. Figure 2a shows the trace from

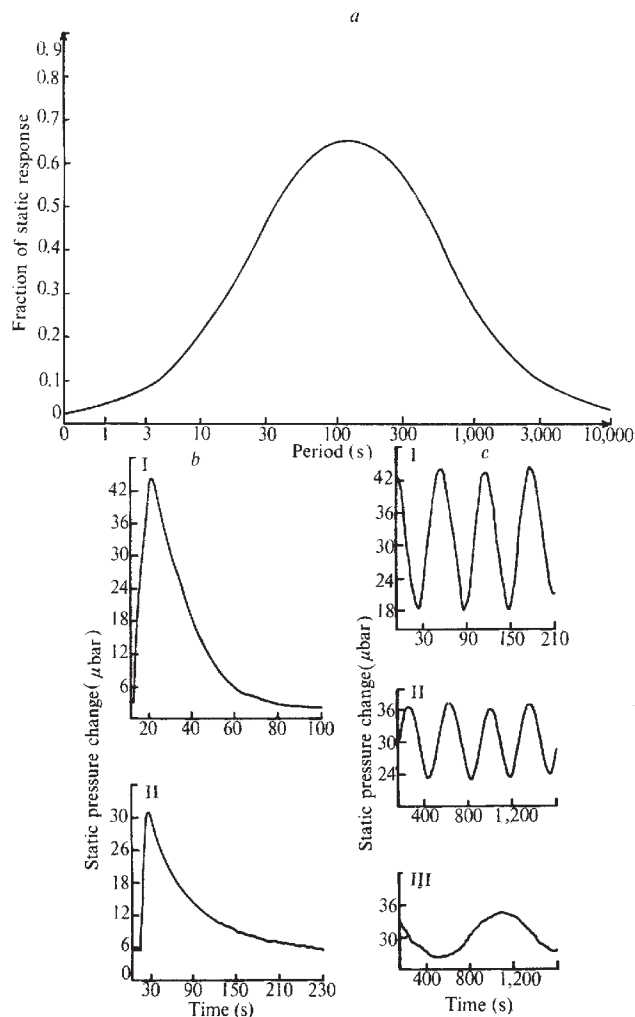


Fig. 1 a, Theoretical response curve for the microbarograph with fast leak set at 7 s and slow leak 80 s. b, Typical exponential outputs used to find τ_1 : I, valve setting 200°, chart speed 60 mm/M, τ_1 13.5 s; II, valve setting 240°, chart speed 20 mm/M, τ_1 80 s. c, Response of instrument to a sinusoidal pressure change of 30 μ bar, but different periods: I, 63 s; II, 380 s; III, 1,140 s. Pass band (3 dB points on response curve) approximately 60 s to 300 s.

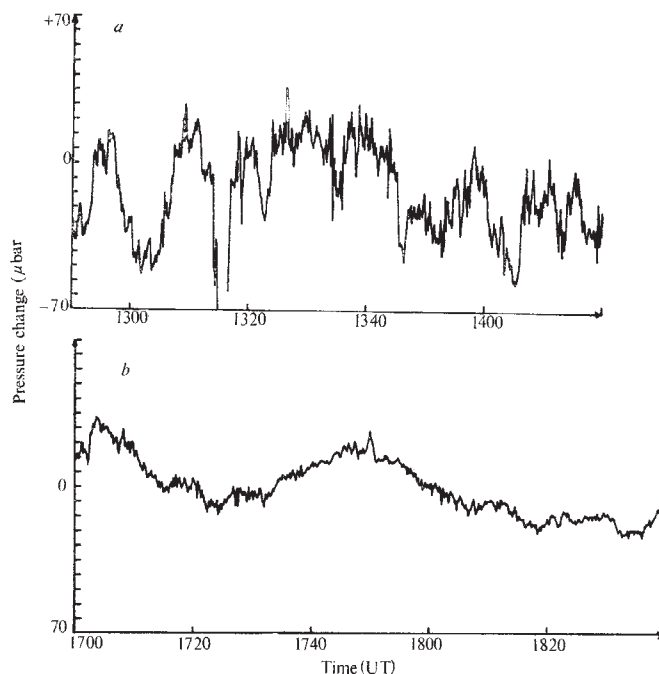


Fig. 2 a, Microbarograph record for the period 1250 UT to 1420 UT on June 30, 1973. b, Microbarograph trace in quieter conditions on a previous day. Sensitivity and pass band as for the observation on June 30.

1250 to 1420, and Fig. 2b illustrates a much quieter period on another day. There are two principal features on the trace; first, a sine wave starting at about 1257 UT with period 15 min and amplitude 60 μ bar. It is possible that this is one of a train, though the earlier ones are smaller and difficult to make out in the noise. At 1314 UT a sharp decrease in pressure was recorded which sent the instrument off scale for about 2 min, after which the trace returned to its original noisy pattern.

These two features could just possibly be noise, but no other such isolated feature has been recorded during 50 d almost continuous operation. There were no meteorological fronts in the vicinity at the time of observation, and the duration was far too long to be the product of a sonic boom⁸.

Assuming great circle tracks and a speed of propagation of 320 m s⁻¹, the gravity wave should have reached the instrument after 1353 UT. It therefore seems that the most significant features recorded on the microbarograph could not be due to the eclipse. We have no evidence of any short period eclipse induced gravity waves, nor the long period waves predicted by Chimonas and Hines¹, though the instrument could not really be set up with a broad enough pass band to take in the lowest frequencies. It is hard to see how, in view of its timing, the isolated disturbance noted above could be connected with any effect of the eclipse, and we thus confirm the conclusion of Schödel, Klostermeyer and Röttger⁹.

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Rapid Variations Observed during the Total Eclipse of the Sun on June 30, 1973

FOR nearly a century the solar corona has been considered responsible for the interplanetary corpuscular radiation and in particular large streamers have been thought to be the main source of this radiation.

But during the past ten years many expansion models (solar wind) have been put forward; in these an important part is played by the hydrodynamical acceleration of the coronal plasma by solar gravity (gravitational nozzle). These models, however, always neglect one of the most important characteristics of the solar corona: that it is heterogeneous.

Information obtained during a total eclipse of the Sun (when the solar corona may be observed in critical regions where the specific acceleration of the plasma ($r \approx 3.5 R_{\odot}$) occurs) is naturally limited as the observation time was very short. During the total eclipses of July 10, 1972, and June 30, 1973, however, we attempted an experiment to observe the solar corona with the best spatial resolution possible at different moments separated by about 2 h. In fact the weather conditions were favourable during the most recent eclipse.

Two cameras, equipped with the same achromatic lenses $F=3,000$ mm and $\phi=200$ mm and the same compensators of brightness gradients (radial neutral filter), worked well—one in Mauritania near Atar, the other in Tchad near Moussoro. The total eclipse could be seen from these two observation points with a time difference of 1 h 30 min. Several plates were taken with the same instrumental para-

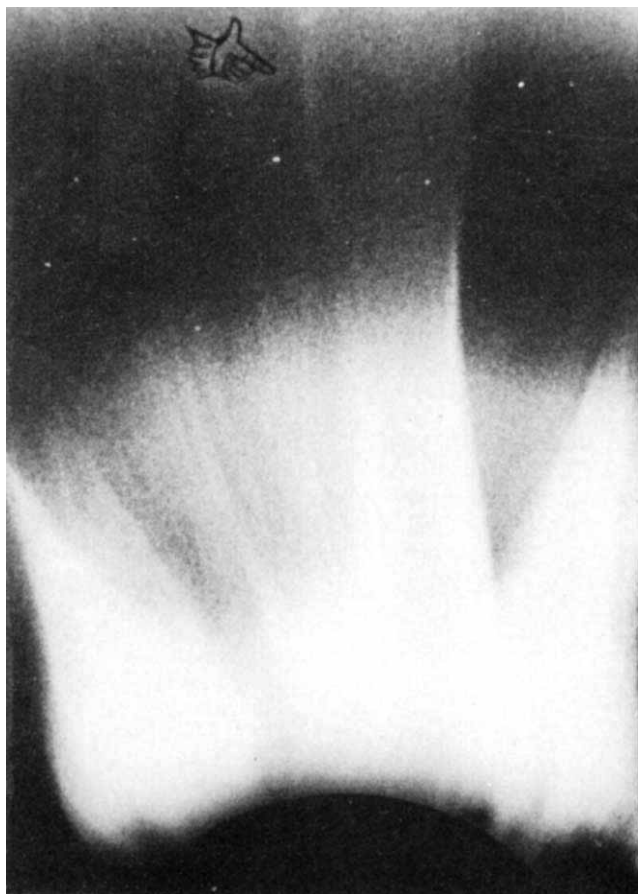


Fig. 1 Contrasted photograph of part of the plate taken at 1213 UT at Moussoro (Tchad) by the team of the Institut d'Astrophysique, Paris, showing the 'detachment' phenomenon of part of the large coronal streamer.

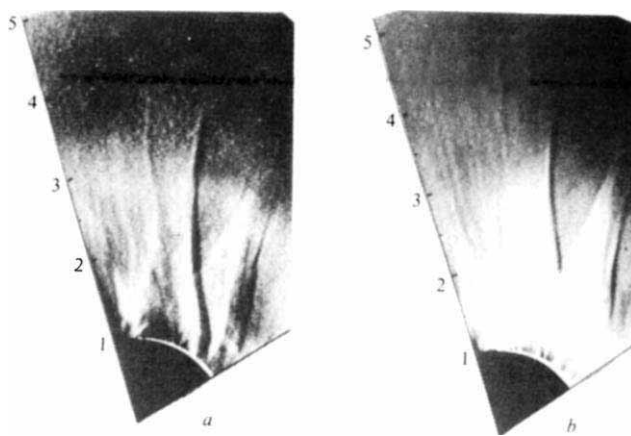


Fig. 2 A photograph made up of two plates taken with the same observational instruments at Atar, Mauritania, at 1046 UT (a, team from the University of Kiev, USSR) and at Moussoro at 1213 UT; (b, team from the Institut d'Astrophysique, CNRS, Paris). In order to increase the contrast in the outer regions the derivative of each photograph was obtained by compositing the negative and the positive copies together, shifted by $\Delta = 6 \times 10^{-2} R_{\odot}$ perpendicular to the streamer.

meters. Their detailed comparison will be the subject of future publications. In addition some weighted plates were obtained by a third group in Kenya with another instrument; its performance was not as good as that of our cameras ($F=1,550$ mm).

Most of the plates were made with Kodak films (18×24 cm); the exposure times were 10 and 20 s. Weather conditions were not excellent (on account of dust, nebulosity and so on). Nevertheless the exceptional length of the eclipse (maximum duration 7 min) enabled us to study the outer regions of the corona ($r \geq 3 R_{\odot}$) with plates taken in the neighbourhood of the centre of the total eclipse, without being hindered too much by the sky background. We increased the nominal contrast ($\gamma \approx 1$) when developing the films so as to improve the resolution of the structural details. On all plates we managed to identify several stars of magnitude 6 and 7 which helped the comparison.

The plates obtained show many variations in structural details. A preliminary survey has nevertheless revealed a region situated in the outer corona ($r > 3 R_{\odot}$) to the west, where these variations have an exceptional characteristic (see Fig. 1). The end of the stalk of a large streamer seems to be a 'detachment' from the corona.

In fact in the region that we are considering, the plate obtained at 10.46 h clearly shows the end of the streamer—very thin and stretched out to $r \lesssim 3.85 R_{\odot}$ (radial distances are measured perpendicular to the line of sight). On the plate taken at 12.13 h, this part of the streamer has moved to a position which varies between 450,000 and 750,000 km ($r \lesssim 4.9 R_{\odot}$) according to the part of the streamer considered. It is then easy to calculate that the 'mean velocity' of the movement of the different parts of the streamer which are 'detached' varies from 90 to 140 km s⁻¹ according to their radial distances.

The information which has been gathered seems unique in the history of astronomy. This is not surprising, for observations with good spatial resolution ($10''$) of the outer corona have only been made over very short periods of time. It is difficult, under such conditions, to evaluate correctly the frequency of the observed phenomenon. At least it would be reasonable to consider this phenomenon as typical of the evolution of the extremity of large coronal streamers. Probably the explanation is in terms of an instability phenomenon, linked with the behaviour of the streamer in the neighbourhood of the critical point ($r \approx 3.5 R_{\odot}$). We also note that this phenomenon seems to be connected with the evolution of a tangential discontinuity of the coronal plasma.

On the other hand, we do not exclude the possibility that the phenomenon of 'detachment' is connected with the propagation along the lines of force of the magnetic field of a perturbation which would have its origin in the chromosphere.

It seems difficult from these results to reconcile this description of the expansion phenomenon of the outer solar corona with the hypothesis, accepted up till now, of quasi-stationarity at least in the ranges considered here, which are of the order of 10^5 km.

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Possibility of Determining Intergalactic Gas Density by Radio Observations of Flares of Remote Sources

DETERMINATION of the mean gas concentration in intergalactic space (N_{mg}) is an important problem which is still far from being solved. One may hope finally to determine the value of N_{mg} in different indirect ways (see, for example, ref. 1). At the same time there is a possibility of finding N_{mg} directly by measuring the radio signal retardation time $\tau(\nu) = t(\nu) - t(\infty)$ for a frequency ν , where $t(\infty)$ is the time of the arrival of a signal of a rather high frequency. In a completely ionized gas the refractive index $n = 1 - (e^2 N / 2\pi m \nu^2)$, the group velocity $u = cn$ and

$$\tau(\nu) = L/u - L/c = e^2 N_{\text{mg}} L / 2\pi m c \nu^2 = 1.34 \times 10^{-3} (N_{\text{mg}} L / \nu^2) \text{ s}$$

where L is the distance to the signal source assuming $(1-n) \ll 1$; the accuracy of the expressions used is clear (for example, from ref. 2). For our Galaxy, at not very small galactic latitudes, $\int N_g dl \cong N_g L_g \sim (10^{20} \text{ to } 10^{21}) \text{ cm}^{-2}$. Therefore at $N_{\text{mg}} \sim 10^{-5} \text{ cm}^{-3}$ the product $N_{\text{mg}} L > N_g L_g$ if $L > 10^{25} \text{ to } 10^{26} \text{ cm}$. Thus, for extragalactic sources at more than 3 to 30 Mpc signal retardation in intergalactic space at $N \gtrsim 10^{-5}$ is greater than in the Galaxy and could be measured (I note, as an example, that at $N \sim 10^{-5}$, $\nu \sim 3 \times 10^7 \text{ Hz}$ and $L \sim 10^{25}$, the time $\tau \sim 100 \text{ s}$). To apply this method, which is widely used in the case of pulsars³, there must, however, exist rapidly varying and powerful extragalactic radio emission sources; these have not yet been found.

I wish to draw attention to the possible appearance of suitable signals produced in the flares of supernovae or some other sources. I have in mind not entirely hypothetical considerations, which have already been widely discussed, but events connected with the cosmic γ -ray flashes recently discovered^{4,5}. (N. S. Kardashov mentioned (personal communication) that the possibility of observing radio emission correlated with the appearance of 'optical' supernovae in other galaxies has already been discussed. But, both by the conditions of usual optical observations and by the very nature of the γ bursts observed, they do not correlate with optical supernovae (see

also ref. 4) but do correlate with the radio bursts discussed here. Nevertheless, supernovae or unusual supernovae may be the sources of the γ bursts under discussion.) These flashes (with characteristic duration of the order of 0.1 to 10 s) are possibly caused by supernovae flares (at the initial stage of their evolution) at 1 to 10 Mpc (in the near future such events will be evidently observable also up to distances of 100 Mpc (ref. 5)). If such an explanation⁴⁻⁶ is correct, then in a flare an energy $W \sim 10^{47} \text{ to } 10^{48} \text{ erg}$ is transferred to γ rays and at least this energy is likely to be contained in the upper envelope of the star expanding at velocity $v \sim c$. In the course of further expansion this envelope must radiate mainly longer waves and particularly radio emission. The most effective processes are probably magnetobremstrahlung (some magnetic field is certainly present inside the envelope) and different collective (plasma) mechanisms of radio emission.

It is hardly possible to estimate reliably the power or the total energy of radio emission. I do not see, however, any reason to deny the possibility of an energy output in a radio band of, say, $W_r \sim 10^{-6} W \sim 10^{41} \text{ to } 10^{42} \text{ erg}$. For a distance $L \sim 30 \text{ Mpc}$ this corresponds to the density $S = W_r / 4\pi L^2 \sim 10^{-12} \text{ to } 10^{-11} \text{ erg cm}^{-2}$ or for a band $\Delta\nu \sim 10^9$ it corresponds to the spectral density $10^{-21} \text{ to } 10^{-20} \text{ erg cm}^{-2} \text{ Hz}^{-1}$. These figures are, of course, arbitrary but help us to realise that in the region 30 to 300 MHz, particularly interesting for measurement of $\tau(\nu)$, the radio emission flux may be sufficient for detection. It seems reasonable to use several points of observation (see refs 7 and 8) and, of course, with correlation of radio measurements with γ measurements on satellites. The possibility is not excluded that it would be possible to register γ bursts even on the Earth's surface by observing some perturbations in the upper atmosphere induced by these bursts (primarily, I have in mind optical flashes arising at a considerable part of the Earth). Radio flashes should evidently retard by comparison with γ flashes (both on account of the influence of plasma during propagation and on account of the generation conditions), and radio signals of lower carrier frequency (according to the conditions of propagation and probably also to those of generation) should retard by comparison with signals of higher carrier frequency. If radio signals seem to be polarised, measurement of the polarisation parameters at different frequencies will yield additional information and first of all the mean value of $N(H)$ along the line of sight. (Here H is the projection of the magnetic field at the direction of observation.) Since the connection of the observed γ events with supernova flares is not yet proved, I note that the appearance of radio flashes is also probable for some other possible sources and will help to clarify their nature (for example, if these sources are galactic, that is, they are some flaring stars, the retardation $\tau(\nu)$ may prove insignificant).

Although the appearance of the type of sufficiently powerful radio signals discussed cannot be guaranteed, the corresponding observations seem fully justified.

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BIOLOGICAL SCIENCES

Distribution of Microorganisms in Hailstones

LARGE numbers of microorganisms are present in the residues of hailstones, together with other insoluble particles. As the hailstone is the surviving testimony of the life history of a storm and as the organic material can be considered a natural 'tracer' of the ingested air mass, it is natural to investigate whether any information about the conditions in which hailstones grow can be deduced from the internal distribution of microorganisms.

Cloud physicists are in fact in search of new parameters with which to determine the growth conditions in the cloud solely from an examination of the hailstones collected. Aerobiologists, on the other hand, are interested in the aerosolisation, diffusion in the atmosphere, transport and removal of aerospora; in this cycle the process of in-cloud scavenging and washout by precipitation elements plays an important role for particles of low fall velocity ($\sim 0.3 \text{ cm s}^{-1}$ for a $10 \mu\text{m}$ particle of density 1 g cm^{-3}). Thus there are two good reasons for investigating the presence of microorganisms in precipitations. Surprisingly, however, the subject remained almost unstudied in detail even though it was discovered a long time ago¹.

As former work dealt with the melting of complete hailstones² our interest centred on the local distribution of germinating microorganisms inside hailstones relative to the internal features. Non-germinating particles (pollen and fungal spores) have also been identified and counted with a microscope. We chose for the test a giant ellipsoidal hailstone, largest dimension about 7 cm, which was collected during the summer of 1969 near Garden City, Kansas.



Fig. 1 Hailstone slice, as seen in reflected light, showing the sequence of opaque-bubbly and transparent layers.

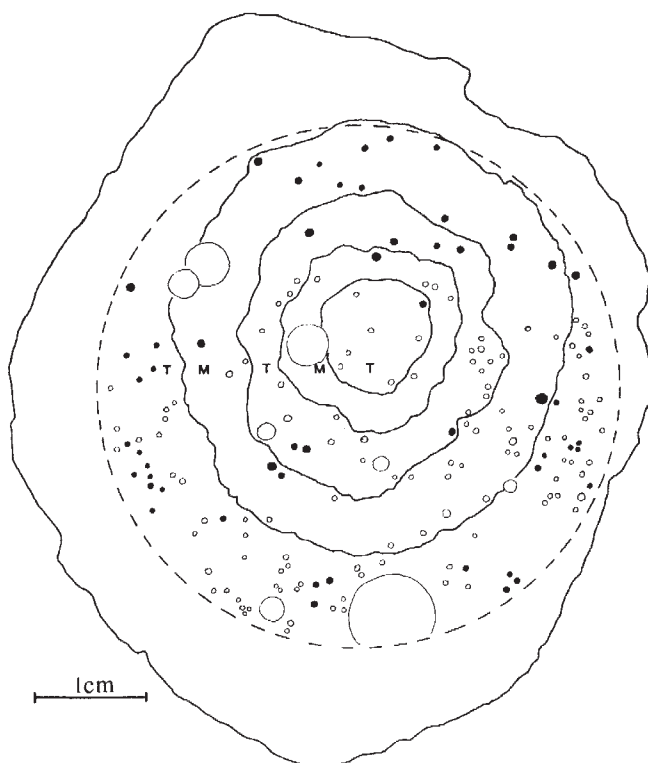


Fig. 2 Distribution of bacterial and fungal colonies grown on the concentric series of filters; the black disks represent colonies grown 24 h later than those represented by empty circles. The map has been superimposed on the outlines drawn from Fig. 1 and shows layers of opaque (M) and transparent (T) ice. The hatched circumference defines the area that has been examined.

A slice 2 mm thick was sawn off the main section and prepared with a microtome. All operations, including working, melting and filter preparation, were carried out in sterile conditions. The slice was then photographed in crossed polaroids and reflected light. The external layer was washed with sterile water to eliminate possible contamination and the slice was placed on a concentric array of membrane filters (pore size $0.45 \mu\text{m}$) of different diameters in a sterilised filter holder (diameter 142 mm) connected to a vacuum pump.

The particles deposited on the filters maintained their original locations on account of the very slow melting of the bottom of the slice. The filters were then separated and placed in different Petri dishes containing a nutritive medium³ and incubated at 24°C . Other slices of the same hailstone were melted and filtered, and the residues observed with the microscope. The results are shown in Figs 1 to 3. Figure 1 is a photograph of the slice in reflected light, which indicates the alternating sequence of opaque-bubbly and transparent layers. A map of the colony distribution has been superimposed (Fig. 2) on the outline of the internal features of the hailstone as deduced from Fig. 1. A graph of the radial distribution of micro-organism concentration for the different layers of the stone is shown in Fig. 3. There is a general decrease in the concentration of colonies towards the centre of the slice. More surprising, however, is the remarkable correspondence with the internal features: in the opaque layers there is only one-third to one-half of the colonies found in the transparent layers.

The increase with radius of the number of germinating colonies is in agreement with the results of Rosinski⁴ for giant aerosol particles in different hailstones. Possible explanations of this result are in terms of the increase of

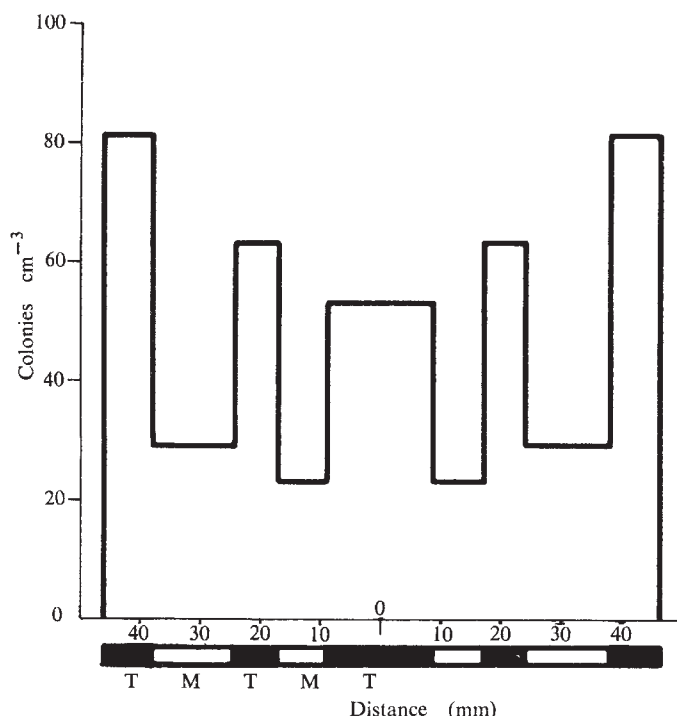


Fig. 3 Histogram of the distribution of colonies grown on the areas M and T represented in Fig. 2.

particle concentration in the accreting droplets and the intense evaporation of the hailstone, which is much warmer than its environment during the final stage of growth. It seems more difficult, however, to interpret the correspondence of the colony concentration with the sequence of layers. The lower density of the opaque-bubbly layers cannot alone explain the lower content of colonies. In fact measurements of local density in hailstones⁵ show, for this type of hailstone, a maximum reduction of 10% in density due to the presence of air bubbles. On the other hand, the high collision efficiency of large cloud droplets for particles of the size of airborne microorganisms suggests that organic material is transferred to the hailstone more through capture of droplets than through direct aerodynamic capture. If so, the opaque layer and the lower particle content would both have been consequences of accretion with a narrower spectrum of cloud droplets. The possibility that the variations in particle concentration are due to variation in the evaporation rate during the hailstone growth cannot, however, be excluded. Rejection of particles by the advancing ice-water interface during the final freezing is important only in very spongy growth, which is not the case for the hailstone examined.

It seems, however, that a necessary feature of further investigations will be the measurement of the aerospora profile in the environmental air together with knowledge of the circulation pattern of the storm, especially concerning the updraft levels.

As far as the identification of germinated colonies is concerned, only 3% are fungi (two colonies of Ascomycetes, one of which is a *Penicillium* sp., and three colonies of Moniliales, one of which is a *Cladosporium* sp.); the remainder is made up of a single type of Gram-positive streptococcus.

The aerospora composition seen only at the microscope of other slices from the same hailstone is quite different: *Alternaria*, 30%; *Cladosporium*, 9%; *Torula*, 1%; other Dematiaceous fungi, 26%; basidiospores, 4%; other spores, 23%; *Pinus* pollen, 5%; and other pollen grains, 2%. The concentration of the total aerospora is four times that of the

germinating colonies. Apart from mortality due to stress inside the hailstorm, some of these organisms could have been damaged during long storage in the freezer, including the air travel to Europe in a dry-ice container. This is confirmed by the observation that the incubation time before germination was longer for this than for other 'fresh' hailstones (4 to 5 d instead of 1 to 2 d).

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Phagocytic Release and Activation of Human Leukocyte Procollagenase

DESTRUCTION of collagen-containing structures occurs in those inflammatory disease states which are characterised by the infiltration of large numbers of granulocytic leukocytes¹. The degradation of collagen fibrils is probably mediated by a specific mammalian collagenase, since native collagen at physiological pH and temperature is resistant to hydrolysis by other proteolytic enzymes². Active collagenase initially identified in culture fluids of tadpole tail³ has since been demonstrated in cultures of human skin⁴, gingiva⁵, and rheumatoid synovium⁶. Furthermore, collagenase has been demonstrated in human rheumatoid synovial fluid and extracted directly from human granulocytic leukocytes^{7,8}. Recently, an inactive collagenase precursor was extracted from human leukocytes⁹. We now report that a collagenase precursor is liberated in large quantity from viable cells during phagocytosis of aggregated human γ globulin (AHGG). In addition, we show that the neutrophil is the principal source of this proenzyme, with only small amounts present in lymphocytes or lymphoblasts. The proenzyme can be activated by trypsin or rheumatoid synovial fluids but not by osteoarthritic synovial fluid.

Human leukocytes were isolated by dextran sedimentation of peripheral blood obtained from healthy volunteers negative for rheumatoid factor. The leukocyte-enriched fraction consisted on the average of 50% neutrophils, 42% lymphocytes, 6% monocytes and 1–2% eosinophils. A neutrophil-free population of mononuclear cells (95% lymphocytes, 5% monocytes) was isolated by isopycnic sedimentation in Ficoll-hypaque¹⁰. The purified mononuclear cell population was either used immediately or cultured in Medium 199 with HEPES buffer containing 15% foetal calf serum, penicillin, streptomycin and 20 μ l ml⁻¹ of phytohaemagglutinin-P (PHA-P) for 3 d at 37° C. Transformation to large lymphoblasts by PHA was monitored by incorporation of ³H-thymidine. Cell lysates (mixed leukocytes, lymphocytes or PHA-transformed lymphocytes) were prepared by suspending each of the various cell populations in Hanks' balanced salt solution (BSS) containing 1% glucose. The suspensions were frozen and thawed ten times and homogenised in a Dounce homogeniser. The resultant homogenates were dialysed, lyophilised and stored at -70° C for as long as 1 week. The lysates were reconstituted with distilled H₂O

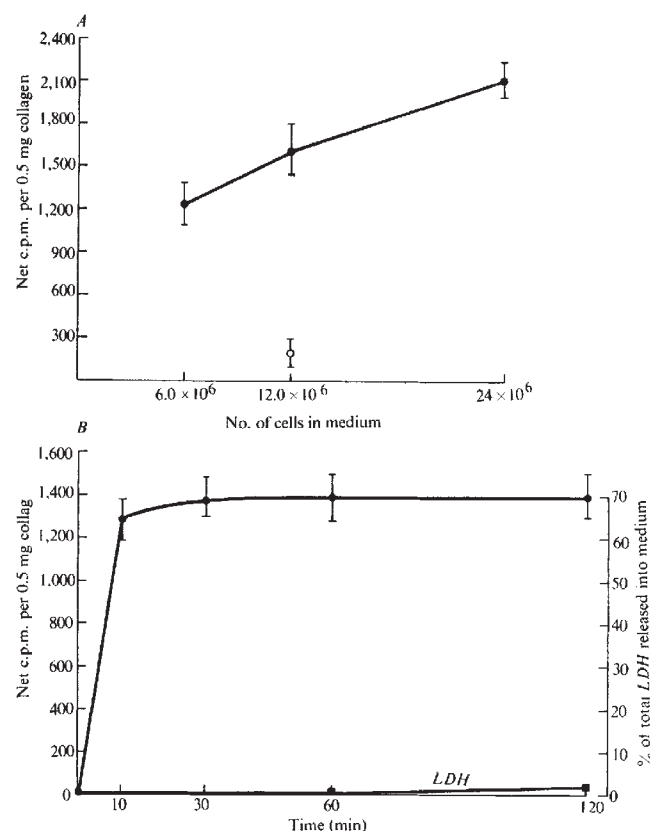


Fig. 1 *A*, Release of procollagenase from mixed leukocytes during phagocytosis of aggregated human IgG (AHGG). Supernatants were activated by exposure to rheumatoid synovial fluid as detailed in text. ●, Cells incubated with AHGG. ○, Cells incubated with buffer only (no AHGG). Each point represents mean $\pm$ s.e. *B*, Effect of incubation time on the release of procollagenase from mixed leukocytes during phagocytosis of AHGG. Supernatants were activated by $30 \mu\text{g ml}^{-1}$ of trypsin and the reaction terminated with 10^{-3} M TLCK. ●, Mean collagenolytic activity $\pm$ s.e. after trypsin. ■, Percentage of total LDH released into medium.

and centrifuged at $25,000g$ for 15 min before assay for collagenolytic activity or activation. In experiments involving phagocytosis, triplicate samples of $5\text{--}25 \times 10^6$ leukocytes were suspended in Hanks' BSS with 1% glucose (pH 7.5) containing 2% AHGG¹¹. Following incubation for 1–2 h at 37°C , an aliquot of the supernatant was utilised for estimation of lactic dehydrogenase (LDH) activity¹² and the remainder was dialysed against water at 4°C for 24 h, lyophilised and reconstituted when needed in 0.1 M Tris buffer. Whole cell lysates or the supernatant from phagocytosis experiments were activated by exposure at $30.0 \mu\text{g ml}^{-1}$ of TPCK trypsin (Sigma) for 1 h at 25°C . The trypsin reaction was terminated by addition of 10^{-3} M N- α -p-tosyl-L-lysine chloromethyl ketone (TLCK, Sigma). In other experiments whole cell lysates or phagocytosis supernatants ($400 \mu\text{l}$) were mixed with $25 \mu\text{l}$ of osteoarthritic or rheumatoid arthritic synovial fluids (obtained by knee puncture and supplied by Dr M. Ziff, Southwestern Medical School).

^{14}C -Labelled native neutral salt-soluble collagen was prepared by the method of Kang *et al.*¹³. Before use ^{14}C -collagen was reconstituted as a 0.2% gel according to the method of Gross¹⁴. The labelled collagen had a specific activity of 6,400 c.p.m. mg^{-1} collagen. To assay for collagenolytic activity, an aliquot of the sample was added to $250 \mu\text{l}$ of a 0.2% collagen gel (containing 0.5 mg collagen) to which had been added $200 \mu\text{l}$ of Tris- CaCl_2 , pH 7.4. After incubation for 18 h at 37°C , the reaction was terminated with 10^{-2} M EDTA and the mixture centrifuged at $15,000g$ for 30 min. A 0.5 ml aliquot was added to 10 ml of scintillation fluid (Insta-gel, Packard) and analysed for ^{14}C activity. In addition

to appropriate buffer blanks, all experiments included $35 \mu\text{g ml}^{-1}$ of trypsin to quantify the extent of non-specific gel breakdown. An Ostwald viscometer with a flow time for water of about 95 s at 22°C was used for relative viscosity measurements. Rheumatoid-fluid-activated procollagenase (obtained from 10×10^6 cells during phagocytosis of AHGG) caused a 30–35% decrease in viscosity during 24 h of incubation.

Table 1 Presence of Procollagenase in Human Leukocytes and its Activation by either Rheumatoid Synovial Fluid or Trypsin

Type of cell lysate	Activator	Collagenase activity net c.p.m.
—	Trypsin ($35 \mu\text{g ml}^{-1}$)	484 ± 25
—	Trypsin ($35 \mu\text{g ml}^{-1}$) + 10^{-3} M TLCK	65 ± 8
Leukocytes * (15×10^6)	None	98 ± 15
Leukocytes (5×10^6)	Trypsin ($30 \mu\text{g ml}^{-1}$)	667 ± 80
Leukocytes (10×10^6)	Trypsin ($30 \mu\text{g ml}^{-1}$)	$1,450 \pm 120$
Leukocytes (15×10^6)	Trypsin ($30 \mu\text{g ml}^{-1}$)	$2,007 \pm 150$
Lymphocytes † (5×10^6)	None	0
Lymphoblasts-PHA (5×10^6)	None	55 ± 5
Lymphocytes (15×10^6)	Trypsin ($30 \mu\text{g ml}^{-1}$)	670 ± 10
Lymphoblasts-PHA ‡ (15×10^6)	Trypsin ($30 \mu\text{g ml}^{-1}$)	650 ± 20
—	Osteoarthritic fluid	168
—	Rheumatoid fluid	145
Leukocytes (30×10^6)	Rheumatoid fluid	2,827
Leukocytes (30×10^6)	Osteoarthritic fluid	113
Bacterial collagenase ($10 \mu\text{g ml}^{-1}$)	—	$3,209 \pm 50$
Bacterial collagenase ($0.1 \mu\text{g ml}^{-1}$)	—	$2,944 \pm 25$

The buffer blank of 250 c.p.m. was subtracted from all samples.

* Mixed leukocyte suspension consisted of 50% neutrophils, 42% lymphocytes, 6% monocytes and 1–2% eosinophils.

† Lymphocyte population was free of neutrophils and consisted of 95% lymphocytes and 5% monocytes.

‡ PHA transformation of lymphocytes was monitored by ^3H -thymidine incorporation. PHA-transformed cells contained 15,000–20,000 c.p.m. per 5×10^6 cells, while normal lymphocytes did not incorporate significant amounts of ^3H -thymidine.

Control ^{14}C -labelled collagen gels incubated for 18 h at 37°C released about 250 c.p.m. (Table 1) and incubation with $35.0 \mu\text{g ml}^{-1}$ of trypsin caused the additional release of 484 c.p.m. The addition of 10^{-3} M TLCK almost totally inhibited the release of ^{14}C -peptides by trypsin. In another experiment a solution of $40.0 \mu\text{g ml}^{-1}$ of trypsin and 10^{-3} M TLCK had no detectable proteolytic activity when assayed against a 0.1% solution of azacoll for 40 min at 37°C . There was a linear increase in ^{14}C release from the gels when the trypsin-activated lysates from 5, 10, or 15×10^6 leukocytes were used. Lysates of normal lymphocytes, or PHA-stimulated lymphocytes, did not induce significant ^{14}C release. In the presence of trypsin the lysate of 15×10^6 lymphocytes or lymphoblasts solubilised about 20% of the ^{14}C -collagen gel which is equivalent to the release observed with 5×10^6 mixed leukocytes. Since the lymphocytes were a pure population and the leukocytes were composed of about equal numbers of granulocytes and mononuclear cells, it is apparent that the granulocytes contained approximately six times more proenzyme than did the lymphocytes.

Various arthritic synovial fluids were incubated with lysates from 30×10^6 leukocytes. Rheumatoid fluid used alone released 145 c.p.m., but when mixed with non-trypsin-treated leukocyte lysates 2,827 c.p.m. were released (Table 1). Osteoarthritic fluid did not activate the lysate procollagenase. A repeat of the experiment using two different arthritic fluids produced essentially the same results. Solutions of 0.1– $10.0 \mu\text{g ml}^{-1}$ of purified bacterial collagenase (Worthington) solubilised 78%–100% of the gel within 18 h (Table 1).

To determine whether human leukocytes could release

procollagenase during the process of phagocytosis, intact cells (triplicate determinations) were incubated with AHGG for 2 h at 37° C. Aliquots of the supernatants were removed, incubated with rheumatoid arthritic synovial fluid and the mixture was added to ¹⁴C-labelled collagen gels. There was a direct relationship between the number of cells in the medium and collagenolytic activity (Fig. 1A). The supernatant from 12 × 10⁶ cells incubated with buffer (no AHGG) and then with rheumatoid joint fluid was inactive. To estimate the rate of proenzyme release, a constant number of cells (6 × 10⁶) was incubated in triplicate with 2% AHGG for 10, 30, 60 and 120 min at 37° C. The supernatants from these experiments were activated with trypsin and the reaction terminated with 10⁻³ M TLCK. Maximum proenzyme release occurred within 15 min of exposure of intact leukocytes to AHGG (Fig. 1B). An estimation of cell viability during phagocytosis was obtained by assaying the supernatant for LDH activity. Less than 3% of the total LDH from 6 × 10⁶ leukocytes was released into the medium during the 2 h incubation (Fig. 1B), and in addition >95% of these cells excluded trypan blue dye.

A collagenase precursor has recently been demonstrated in organ cultures of tadpole tail¹⁵, mouse long bone and in human leukocytes. In mouse bone, procollagenase has been shown to be activated by limited proteolysis with trypsin, chymotrypsin or purified lysosomes¹⁶. Human leukocyte procollagenase activation was achieved by rheumatoid synovial fluid⁹. We now confirm these studies since lysates of human leukocytes were activated by either trypsin or rheumatoid synovial fluid but not by osteoarthritic fluid. In addition, we show that the neutrophil contains the highest content of the proenzyme, while the normal or PHA-transformed human leukocytes contain approximately one-sixth as much. The collagenase precursor was released from viable human granulocytes in relatively large quantities under physiological conditions during the process of phagocytosis of AHGG. The phagocytic release of various other enzymatically active proteases from human leukocytes including β -glucuronidase, elastase and neutral protease has been demonstrated^{17,18}. There is, however, a significant difference in the kinetics of the release of the procollagenase since maximum lysosomal enzyme release (neutral protease) occurs in 90–120 min¹¹, whereas the procollagenase is probably released in the form of a brief pulse (within 15 min). The fact that the rate of procollagenase release differs so markedly from other granule-bound enzymes suggests that the proenzyme may be localised in a different subcellular fraction.

The pannus of the rheumatoid joint has the ability to produce an active collagenase and invade and degrade cartilage *in vitro*¹⁹. This implies that collagenase may be a significant factor in the pathology of rheumatoid arthritis. The demonstration of a collagenase precursor released during phagocytosis of aggregated human IgG and activated by a proteolytic enzyme or rheumatoid arthritic synovial fluid may also contribute to the joint degeneration seen in rheumatoid arthritis, a disease characterised by a large immigration of leukocytes to the inflamed joint. Leukocytes have been shown to phagocytose immune complexes present in the rheumatoid joint²⁰. The *in vivo* release of procollagenase during phagocytosis and its subsequent activation by the rheumatoid synovial fluid could then give rise to active collagenase which could result in the initial split in the collagen molecules of joint cartilage²¹. The neutral protease, which we have already shown is released by the leukocyte during phagocytosis¹¹, would then complete the degradation of the collagen fibrils of joint cartilage. In addition, Harris has shown that human leukocyte collagenase can destroy cartilage collagen, although the rate of degradation is slower when compared to its effect on native reconstituted skin collagen fibrils²².

The failure of osteoarthritic fluid to activate human leuko-

cyte procollagenase is consistent with the hypothesis that osteoarthritic joint destruction may be mediated by physical trauma rather than enzymatic destruction.

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Detection in Normal Plasma of Immunoglobulin resembling the Protein of γ -Chain Disease

TUMOURS of the lymphoid system have from time to time presented protein products apparently unique to the neoplastic process, but found later to occur in inconspicuous amounts in the normal organism. Thus Bence Jones protein was described in the urine of a patient with multiple myeloma in 1848 (ref. 1), found in about 10⁻⁴ times this amount in normal urine 112 yr later², and soon after recognised in both the pathological and normal circumstances to consist of free immunoglobulin light chains^{3,4}. More recent discoveries of undescribed myeloma proteins are apt to be followed rapidly by recognition of their normal analogues⁵⁻⁷. No normal analogue of the protein of the type described in 1963 by Franklin *et al.* in association with a disseminated lymphoid tumour^{8,9} has, however, been described. The tumour was called 'heavy chain disease' because it was seen to be secreting⁹⁻¹¹ an unusual protein related to immunoglobulin heavy chains. Heavy chain disease is now divided into three types¹²: according to whether the associated protein is related to the γ , α or μ class of heavy chains the disease is called γ , α or μ chain disease. The original case was of γ chain disease, and up to August 1972 thirty such cases had been recorded¹³.

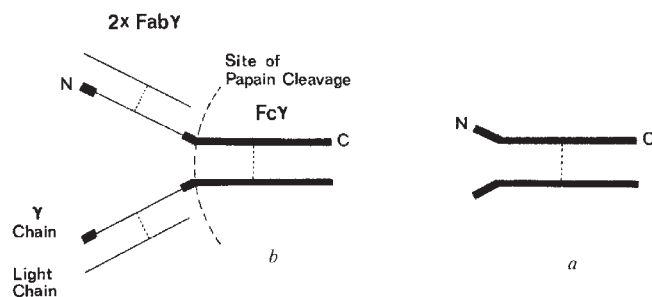


Fig. 1 *a*, The structure found for homogeneous γ CD proteins; *b*, that of human IgG. IgG consists of two γ chains and two light chains, joined by interchain bonds . . . , and cleaved by papain into one FcY and two FabY fragments as depicted. The heavy lines show the sequences represented in γ CD proteins. These have a small N-terminal sequence typical of whole γ chain, then after an apparent deletion of 100–200 amino acid residues a stretch of more than 200 residues embracing the entire C-terminal half; that is, the FcY region. The antigenic structure of γ CD proteins, deriving from the bulk of their sequences, resembles that of FcY.

Studies^{13–15} of a well characterised γ -chain disease (γ CD) protein revealed an N-terminal sequence of eighteen amino acid residues typical of whole γ chains, but a sequence from residue 19 to the C terminus identical with a γ -chain sequence from residue 216 to the C terminus: thus it looked like a γ chain with an internal deletion of 198 residues (Fig. 1). As appears likely from Fig. 1 the γ CD protein had all the FcY antigenic determinants but insufficient FabY sequence to give any FabY determinants.

Other γ CD proteins were soon found with similar but not identical internal deletions. They appear to fall into two groups^{15,16}: one of proteins themselves homogeneous, but with internal deletions varying from one to another; the other of proteins with heterogeneous N-terminal sequences, perhaps resulting from some postsynthetic degradation of proteins resembling those of the first group. The internal deletion has been much discussed with regard to its implications for the molecular genetics of immunoglobulins^{17,18}.

We have sought and found in normal human plasma a protein resembling γ CD protein by three criteria. First, a molecular weight appreciably less than that of IgG (150,000); second, an antigenic structure containing FcY but not FabY determinants; third, N-terminal peptides containing the cyclised residue pyrrolidonecarboxylic acid (PCA), typical of whole γ chains. A preparative procedure was designed to yield only protein satisfying the first two criteria ('FcY-like' protein), and this was then analysed for N-terminal PCA.

Figure 2 illustrates the preparative procedure. Precipitin analysis by the Ouchterlony technique revealed that the isolated FcY-related protein consisted of at least two components. One (FcY-like) contained only FcY determinants. The other ('IgG-like') had both FcY and FabY determinants. The IgG-like protein was removed by passage through Sepharose 4B-(rabbit anti-human FabY), and awaits further characterisation.

Upon analytical centrifugation the FcY-like protein sedimented as a single symmetrical peak, with $S_{20}=3.9S$ in Tris-NaCl. Ouchterlony analysis with anti-FcY serum revealed a reaction of antigenic identity with authentic FcY; there was no reaction with anti-FabY nor with anti-(light chain) sera. Immunoelectrophoresis with the anti-FcY serum revealed a single broad arc of precipitation in the β - γ mobility region.

The protein was examined for N-terminal PCA by a method adapted from Press, Piggot and Porter²¹. A sample of 11.5 mg was fully reduced and alkylated, and digested for 3 h at 37°C with Pronase at pH 8.1 and an enzyme:

substrate ratio of 1:25 (w/w). The digest was run through a column of Dowex 50 (X2, H⁺ form). Unbound material was eluted with water and concentrated by desiccation under reduced pressure. When spotted onto Whatman 3 MM paper it gave a negative ninhydrin reaction and a strongly positive reaction with the hypochlorite/starch-iodide reagent of Rydon and Smith²². When tested by high voltage electrophoresis at pH 6.5 on thin-layer silica gel plates it resolved into three acidic and one very faint neutral spot, all stained by hypochlorite/starch-iodide (Fig. 3).

The presence after these procedures of acidic material staining with starch-iodide and not with ninhydrin has been unique to proteins having N-terminal PCA^{21–24}. Comparison of the mobilities of the spots with those of standard PCA and PCA-containing peptides (Fig. 3) suggests further that the FcY-like protein has yielded PCA, PCA-Val and PCA-Val-Gln. This is the combination yielded by human normal γ chains after the same treatment. Clearly it would be desirable to have an amino acid analysis for each spot: we intend to obtain larger amounts of material to make this possible.

A visual comparison of electrophoretograms derived from FcY-like protein and whole γ chains revealed that the relative intensities of staining of the three spots, all in fact approximately equal, were similar in each case; on the basis of 40% of the γ chain population bearing N-terminal PCA²⁴ approximately 8% of the FcY-like protein appears to bear it.

The above results demonstrate among the FcY-like protein a species which resembles γ CD protein by the three criteria which we set out. We shall call it partially deleted γ chain. Then the FcY-like protein has at least two com-

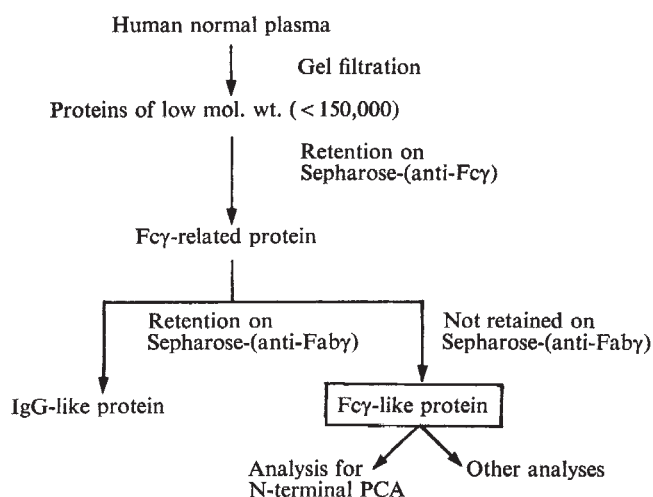


Fig. 2 Preparation and characterisation of FcY-like protein from human normal plasma. Pooled normal human plasma from a transfusion centre was defibrinated, and its proteins fractionated by gel filtration on Sephadex G-100 equilibrated with 0.1 M NaCl–0.02 M Tris-HCl (pH 8.0)–0.001 M Na₂EDTA–0.003 M NaN₃ (abbreviated below to Tris-NaCl). The 4S protein fraction, containing proteins of molecular weight less than 150,000, was passed through diethylaminoethyl cellulose to remove most of the plasma albumin and then concentrated by ultrafiltration. Further purification was by immunoabsorption. Immunosorbents with a Sepharose 4B matrix were prepared according to Cuatrecasas¹⁹, with the IgG fraction of the appropriate antiserum being coupled at pH 6.5. First, the proteins of low molecular weight were passed through blank (activated and deactivated) Sepharose 4B to remove any material adhering simply to the matrix. Next they were passed through Sepharose 4B-(rabbit anti-human FcY), binding all protein antigenically related to FcY. After washing the immunosorbent with buffer the FcY-related protein was eluted with 1 M NH₄OH, pH 11.7 (ref. 20). Some rabbit antibody leached out by this procedure was removed by passage at neutral pH through Sepharose 4B-(sheep anti-rabbit IgG).

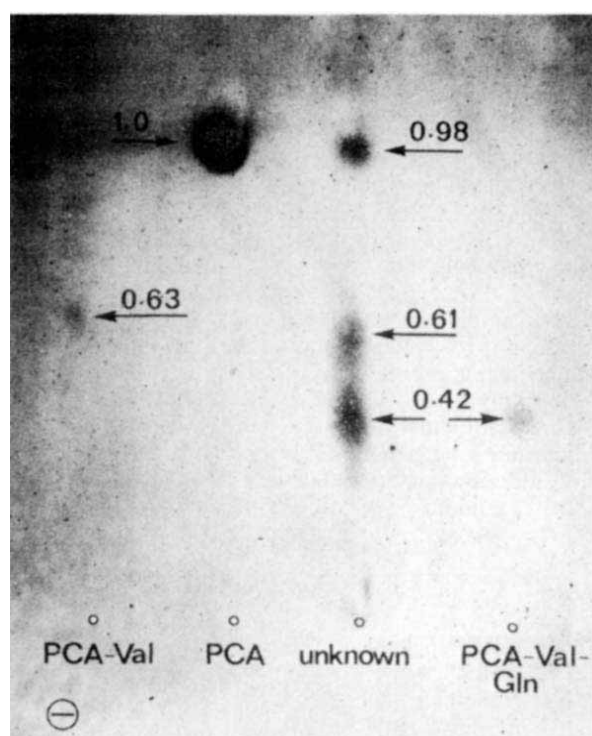


Fig. 3 Analysis by high-voltage thin-layer electrophoresis of material from the Pronase digest of Fc γ -like protein, not retained by cation-exchanger Dowex 50X2. Origins are indicated by small circles at the bottom of the photograph. The numbers are mobilities relative to that of the PCA standard. The three main spots given by the unknown sample have, within experimental error, mobilities equal to those of PCA, PCA-Val, and PCA-Val-Gln.

ponents: catabolically produced Fc γ ²⁶ and partially deleted γ chain. The proportion comprised by the latter, assuming that it resembles normal γ chains in having 40% of its molecules bearing N-terminal PCA²⁴, is very roughly 20%.

The following possible artefacts were considered and eliminated. First, that PCA originated from contaminating Faby-containing material. From model experiments we found that the maximum amount of Faby that could have escaped detection by the Ouchterlony technique would not have sufficed to give visible PCA spots on the electrophoretogram. Second, that PCA came from contaminating rabbit IgG. Again no significant contamination was picked up by Ouchterlony analysis, and in any case PCA-Val and PCA-Val-Gln would not be expected. Third, that PCA originated from cyclisation of N-terminal glutamine²⁵, *in vivo* or *in vitro*, in Fc which had arisen from catabolism of plasma IgG. No known IgG sequence²⁷ would also yield PCA-Val and PCA-Val-Gln. Authentic Fc γ put through our preparative and analytical procedure failed to yield any spots on the electrophoretogram. Conversely, significant underestimation of the PCA content due to opening of the ring²³ in the 1 M NH₄OH used for eluting from immunosorbents was eliminated by showing that incubation of γ chains in 1 M NH₄OH for up to 6 h at room temperature made no detectable difference to the yield of PCA.

At this stage we can make no reasonable estimate of the amount of partially deleted γ chain in normal plasma. We seem to have recovered about 2 mg from 12 l of plasma, but the preparative losses were enormous: probably >90%. It should be pointed out that the chance of one of our fifty apparently healthy donors having harboured unsuspected γ CD, with a known world total of only about thirty in 10 yr, is negligibly small.

The presence of normal protein analogous to γ CD proteins does not necessarily imply that each pathological

protein has a precise normal counterpart. For example it might merely reflect the occurrence of certain genetic accidents¹⁸ in normal as well as neoplastic lymphocytes: so that while DNA sequences encoded in pathological proteins should have precise counterparts, deletions need not. We do submit that deletions in γ CD proteins cannot now be attributed to some perturbation peculiar to the neoplastic process.

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Isolation of Fractions rich in Human Y Sperm

RESEARCH into the isolation of human X and Y sperm is now feasible because of the fluorochrome quinacrine that stains the Y chromosome^{1,2}. It is still impossible to identify the Y chromosome in sperm of other mammals, with the possible exception of gorilla sperm³, but when human sperm are stained with this fluorochrome and viewed under fluorescence microscopy, approximately half of them reveal a Y body. The Y body is the distal end of the long arm of the Y chromosome and shows up as a bright fluorescent dot⁴. The ability to identify the Y chromosome makes possible the collection of data on X and Y sperm^{5,6}.

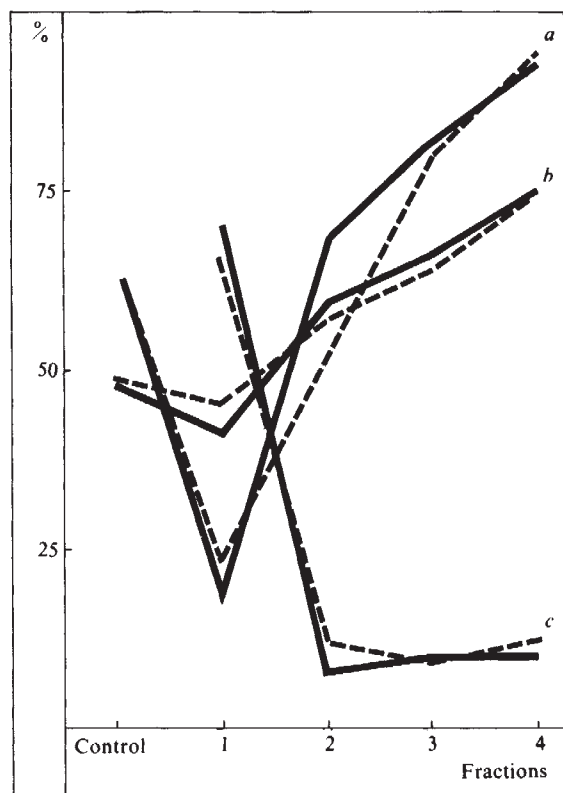


Fig. 1 —, Human sperm layered over a column of 6 and 15% BSA. Fraction 1 is the top sperm layer; fractions 2 and 3 are upper and lower halves of 6% BSA; fraction 4 is 15% BSA. Each line represents the mean of nine experiments with five different donors. (Two similar experiments where ovalbumin replaced BSA gave comparable results.) ---, Human sperm layered over a column of 10, 15 and 25% BSA. Fraction 1 is the top sperm layer; fraction 2 is 10% BSA; fraction 3 is 15% BSA; fraction 4 is 25% BSA. Each line represents the mean of thirteen experiments with five different donors. *a*, Motility; *b*, Y sperm; *c*, recovery.

Human sperm bearing a Y chromosome isolate from sperm bearing an X chromosome when layered over a medium consisting of bovine serum albumin (BSA) or ovalbumin. The basis for isolation is progressive sperm motility⁷, without which isolation of Y from X sperm does not occur. We can isolate repeatedly up to 85% Y sperm, of which 90 to 98% are progressively motile. Aside from considerations of fundamental biology, the isolation procedure is one that adapts to practical application.

The semen comes from seventeen paid volunteers born between 1935–56, some donating semen weekly for 21 months and others for a shorter period. The ejaculates are obtained through masturbation a few hours before use and fulfil the usual criteria for normalcy.

Semen, diluted 1:1 with Tyrode's solution, is centrifuged at room temperature for 15 min at 4,000 r.p.m. and the spermatozoa are then resuspended in Tyrode's at 100×10^6 ml⁻¹. Spermatozoa are checked for percentage motility and the degree of progressive motility⁸ at the start and again in each isolation fraction. Numbers of sperm are determined with a haemocytometer; total sperm recovered do not usually equal 100%, but the error factor is normally no greater than 15%.

The isolation procedure is a simple one in which an albumin in Tyrode's (crystalline BSA or a 25% concentrate in sterile Tyrode's (Sigma); crystalline ovalbumin (Miles-Seravac)) is placed in a glass column. Overlying the albumin is a standardised amount of sperm (50×10^6) in 0.5 ml of Tyrode's. The column is a Pasteur capillary pipette heat-sealed at the point of tapering. The basic one step experiment consists of 0.9 ml of a 3 to 25% concentration of

albumin, pH 7.4 to 7.6, overlaid with sperm. Spermatozoa in the top layer are pipetted off the underlying albumin medium (now also containing sperm) after 1 h. Spermatozoa from top and isolation fractions are then processed through the above-mentioned procedures. A second approach is to use two or three isolation steps, each one with higher concentrations of BSA. This process requires that spermatozoa swimming into the BSA medium be recovered by centrifugation and relayered again (same volume and number) over fresh medium with greater amounts of BSA. Replicate columns are used to increase workable sperm numbers for second and third step isolations. Again, 0.9 ml of albumin medium is used and each step of the experiment is stopped after 1 h; BSA concentrations normally are 6, 10 and 20%. The two or three step procedure is modified to a one step procedure by layering discontinuous gradients of albumin within the same column. A 15% albumin medium (0.4 ml) is overlaid with 0.8 ml 3 or 6% albumin. Spermatozoa in Tyrode's again make up the top layer. Another experiment is carried out by

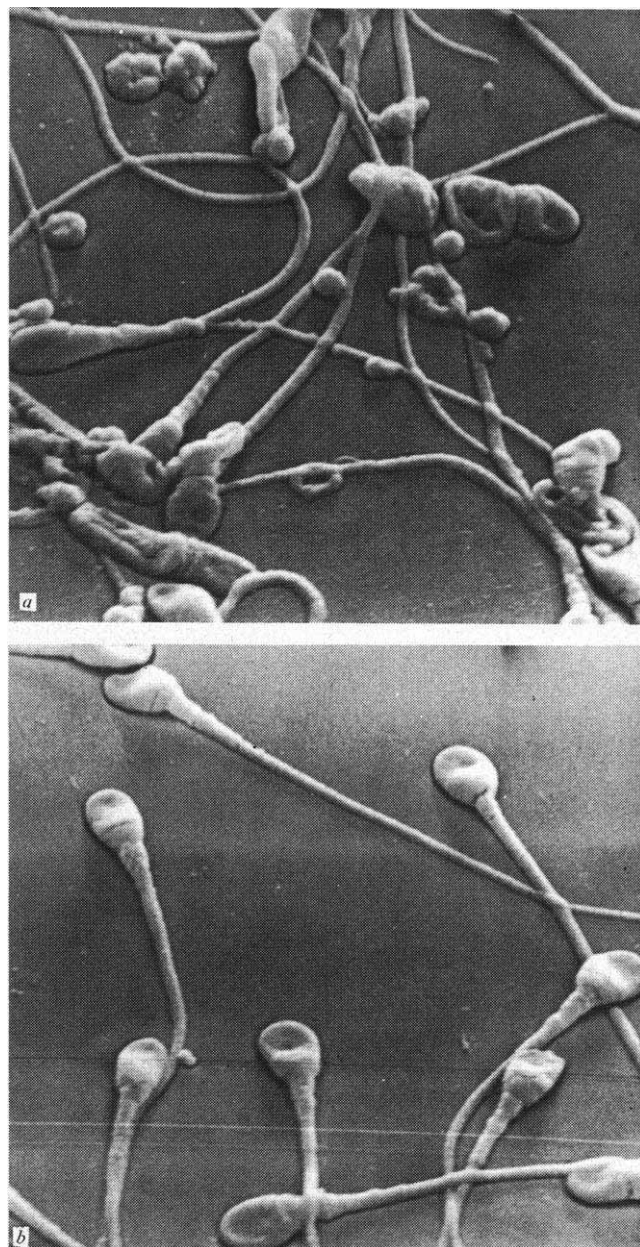


Fig. 2 Scanning electron photomicrographs of human sperm $\times 1,420$. *a*, Fraction 1 (see Fig. 1, solid line); *b*, fraction 4.

layering decreasing gradients of 25, 15 and 10% BSA over one another in 0.3 ml volumes. With multiple layers of albumin, the original sperm layer is still pipetted off after 1 h. The underlying gradients are, however, given additional isolation time of 30 min (two concentrations of albumin) or 60 min (lower two gradients of the three-tiered BSA concentrations). Routinely, spermatozoa in the various isolation fractions are washed 3 to 6 times in Tyrode's to prepare them for staining.

Sperm prepared for counts on Y bodies are air-dried on microscope slides and then stained with quinacrine dihydrochloride (Sigma); in most cases samples are fixed in methanol first. After many months of following literature procedures⁴ we converted to the semiquantitative procedure used by Pearson (personal communication) where the stain is solubilised in methanol at approximately 0.5 to 1.0% concentration. Staining of sperm takes a varying number of minutes, depending largely on the fraction of origin. The slides are then put through two rinses of distilled water, fitted with a sealed cover slip and viewed under a fluorescence microscope. We use a Zeiss photomicroscope equipped with a high pressure mercury vapour lamp, BG 12 and BG 38 exciter filters and 440 and 470 nm barrier filters. Sperm are viewed under oil with a darkfield condenser at $\times 1,000$ magnification.

Table 1 Identifying Human Spermatozoa bearing the Y Chromosome

Total number of sperm counted	40,334
Total number of Y sperm counted	19,564
$\bar{X}$ % Y sperm	48.50
95% confidence limits*	48.26 to 49.31

* Calculated from counts made by one person (34,721 total; 16,939 Y sperm).

Control counts of Y sperm from 116 experiments are listed in Table 1. The arithmetic mean for Y sperm approaches the theoretical value and the 95% confidence limits fit well within values needed for reliable technique. Counts on Y sperm do not differ significantly ($P < 0.05$) within donors, between fourteen donors or according to the time of year. Inability to identify approximately half of the sperm as Y bearing or inability to obtain consistent results would call into question the accuracy of the experimental data. As our method for identifying Y sperm is indirect, this in itself increases the need for reliable control values. We routinely count 600 sperm both from the control and from each experimental fraction.

We are confronted with three explanations for the preponderance of 'Y bodies' in experimental fractions of the isolation column. The differences may be due to human bias, treatment artefact, or actual change in X and Y ratios. We believe we have satisfactory explanations to discredit

the first two possibilities, thereby supporting the third. Human bias (seeing Y bodies that in reality do not exist) can be discounted by always running a control sample, by having more than one person count each sample, and by coding the samples. The two counters (C. N. L. and R. J. E.) have, after considerable experience, compared counts on sixty-one different sperm samples and their results are statistically consistent⁹, that is to say, neither counts more or less Y bodies. Treatment artefacts which might increase what appear to be Y bodies are more difficult to disprove. By exposing sperm to an isolation medium (BSA in Tyrode's solution) without attempting to isolate X and Y sperm, and by using non-motile or low motile sperm where isolation does not occur, we reaffirm our belief that BSA does not, in itself, cause sperm artefacts similar to Y bodies. However, final proof of an increase in frequency of Y sperm must await sexing of offspring conceived of spermatozoa exposed to the isolation process, whether this be in human or other species.

We started our research in X and Y isolation by layering spermatozoa over bentonite. Next we added BSA to the bentonite and eventually BSA without bentonite. We performed more than 110 BSA-only experiments and all succeeded in that progressively motile sperm, of which the majority are Y bearing, moved into the BSA medium. Our point is adequately made in Tables 2 and 3 and Fig. 1. Here we present the results of sixty-four experiments that typify the response of motile sperm to different concentrations of BSA and ovalbumin. We have focused our emphasis on increases of Y sperm in various fractions of albumins. A simultaneous increase in the percentage of motile sperm is also evident (Tables 2 and 3 and Fig. 1). In essence, the process successfully selects a population of spermatozoa that are predominantly Y bearing, almost entirely progressively motile and physically fit. Their homogeneous order results from the selection process where non-motile and many morphologically abnormal forms are excluded (Fig. 2). A logical clinical application of the 'cleaned up' sperm fraction would be in the artificial insemination of subfertile cases.

Sperm recovery (the number that enter albumin fractions) is dependent on progressive sperm motility and albumin concentration plus the added factor of time, which we have not yet adequately tested. The distance sperm must travel is also a factor, but, because the sperm are under gravity, non-motile sperm will with time move in the same direction. We therefore keep sperm recovery to around 25 to 35% by regulating sperm numbers, time and albumin concentration. Figure 1 is offered as evidence that the isolation process is consistent for values obtained, provided sperm have normal progressive and percentage motility. Replicate columns easily provide 10×10^6 to 50×10^6 highly active sperm thought capable of normal fertility. No evidence is available on human sperm fertility, but rabbit sperm subjected to similar isolation processes are fully fertile. In

Table 2 Isolation of Y Sperm with Different Concentrations of Albumin

Composition of isolation fraction	No. donors	No. experiments	% Y sperm*		% Sperm motility*		% Sperm recovery	
			Washed sperm (control)	Top fraction	Washed sperm (control)	Top fraction	Isolation fraction	Sperm recovery
3% BSA	3	7	50 $\pm$ 1	49 $\pm$ 1	57 $\pm$ 1	61 $\pm$ 3	27 $\pm$ 3	85 $\pm$ 2
5% BSA	3	8	49 $\pm$ 1	45 $\pm$ 3	64 $\pm$ 2	69 $\pm$ 2	26 $\pm$ 2	92 $\pm$ 1
6% BSA	3	6	49 $\pm$ 1	47 $\pm$ 2	63 $\pm$ 2	68 $\pm$ 3	19 $\pm$ 3	91 $\pm$ 2
7% BSA	2	2	50	—	64	63	38	83
10% BSA	2	2	48	45	61 $\dagger$	73	25	75
10% Ovalbumin	2	2	46	41	63	50	35	83
20% BSA	1	2	49	46	67	68	35	95
25% BSA (1-2 h)	4	7	48 $\pm$ 1	42 $\pm$ 2	65 $\pm$ 3 $\dagger$	66 $\pm$ 4	25 $\pm$ 5	90 $\pm$ 4

* Mean values $\pm$ s.e. $\dagger$ Lower half of isolation fraction.

Table 3 Y Sperm Yield with a Two or Three Step Isolation Process*

Fractions	Two step isolation, two donors twice †			Three step isolation, one donor twice †		
	% Y sperm	% Sperm motility	% Sperm recovery	% Y sperm	% Sperm motility	% Sperm recovery
Washed sperm (control)	50	63		48	72	
Top fraction No. 1	45	20		46	12	
Isolation fraction No. 1 (6% BSA)	66	93	31	64	85	26
Top fraction No. 2	48	53		—	45	
Isolation fraction No. 2 (10% BSA)	76	98	44	68	95	52
Top fraction No. 3				—	86	
Isolation fraction No. 3 (20% BSA)				85	98	44

* % Y sperm in isolation fractions (Tables 2 and 3) are significantly greater ($P < 0.01$) than corresponding control values.

† Mean values.

this paper we stress the simplicity of this experimental procedure, repeatability of results and practicality of application to humans and perhaps other mammals.

Although we have discovered a practical way to isolate Y sperm, we have not achieved comparable results with X sperm at the same time, even though this finding seems biologically unlikely. The reason is that not all sperm participate in the isolation process and those that do so isolate in a manner that predisposes a like isolation of X sperm. Referring again to the work of Roberts⁷ the gravitational separation of motile human sperm is explained in mathematical models by the superior swimming ability of Y sperm as a function of time. As only 25 to 35% of the original spermatozoa enter the isolation medium and as the isolation process is a factor of time, distance and different viscosities of media, a comparable isolation of X sperm is impossible. Although the top sperm layer does contain an excess of X sperm (Fig. 1), it is only because more of the Y sperm swim out. The excess X sperm fraction is problematical in that it also contains most of the non-motile and abnormal sperm.

Our experimental results and empirical thought lead us to believe that by this process Y sperm isolation is dependent on overlaying of washed motile sperm in a vertical column, thus orientating a swimming movement downward; suspending sperm in a physiological vehicle of density lower than the isolation medium; making the difference in viscosity between sperm vehicle and isolation medium great enough to provide a formidable barrier at their interface; creating the interface such that it separates motile from non-motile sperm, morphologically normal from abnormal forms and starts the Y sperm isolation and extending the swimming distance and isolation time to a point where the now more prevalent Y sperm can continue to outdistance their remaining X sperm counterparts. Therefore, motile Y sperm can be said to have greater ability to penetrate an interface between fluids and between discontinuous gradients of an isolation medium. Y sperm must also swim faster than X sperm under the handicap of swimming in a fluid with, in relative terms, high density and high viscosity. Thus, we believe our concept in this paper breaks tradition with other experimental approaches to the separation of X and Y sperm¹⁰.

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Gastrin Activity in the Chicken Proventriculus

Reaumur and Spallanzani¹ used birds as experimental animals in the early days of digestive physiology; other workers on avian gastric digestion include Cheney², Friedman³ and Long⁴. The bird lacks the gastric antrum analogous to that in the mammalian species and there is a tubularly shaped glandular stomach called proventriculus which produces the gastric juice⁵⁻⁹. Unlike mammals, which have a separate HCl and pepsin-producing cells⁵, birds produce HCl and pepsin from the same gastric cells called oxynticopeptic cells⁵⁻⁹. These cells are arranged in tubular glands organised in lobules deep in the proventricular mucosa with central collecting cavities which open on the mucosal surface through ducts^{5,6}.

A comparison of the gastric secretion of the chicken to man, dog, rat and monkey revealed that the chicken secretes considerably more acid and pepsin in the basal state and following histamine (on a weight basis) than secreted by those mammals listed above⁴.

So far no reports of gastrin activity in the proventriculus nor attempts to extract this hormone from the specialised glandular stomach have been found in the literature. This work was done to detect and assess gastrin activity in extracts of the proventriculus and the duodenum.

Thirty-one healthy male white Leghorn chickens each weighing between 2 to 2.5 kg were used. Ten birds were killed at a time, and their proventriculi and duodenum (10 inch down the small intestine from the junction with the gizzard) were removed. The tubularly shaped proventriculi and the duodenal pieces were cut open and washed clean with water. Each proventriculus weighed between 3.5 to 4 g.

Ten proventriculi and ten duodenal pieces were separately weighed, cut into very small pieces, minced thoroughly and then subjected to the gastrin extraction procedure of Blair *et al.*¹⁰. Thus, there were three occasions when ten birds were killed; the remaining one bird was killed separately and the single duodenum was subjected to the gastrin extraction procedure.

The extracts were dried and their weights per g wet weight of the proventriculus and the duodenum were determined from the weights of each of the three different proventricular

and duodenal extracts. The resulting weights were used to calculate the mean weight of the extract per g wet weight of the proventriculus and duodenum.

Solutions of known concentrations of the powdered extracts were prepared using 0.9% NaCl solution as the solvent and the precipitates obtained after centrifuging for 10 min at 2,500 r.p.m. were discarded.

The saline solutions of the crude extracts were assayed for gastrin activity by using a modification of the continuous stomach perfusion technique described by Ghosh and Schild¹¹. Male Wistar strain rats weighing 180 to 210 g were used. The rats were anaesthetised with urethane (in 25% w/v solution given intraperitoneally at 0.6 ml per 100 g body weight).

The stomachs of the rats were perfused with saline (0.15 M) warmed to the rat's body temperature and the flow rate of the perfusate into the stomach was adjusted to give an effluent volume of 1 ± 0.1 ml min⁻¹. The effluent was collected at 10 min intervals and titrated to pH 8.8 with the Automatic Titrator (Radiometer, Copenhagen) using a centinormal sodium hydroxide as the base. This pH enabled the calculation of the total titratable acid in the collected effluent.

The following graded doses of the saline solutions of the proventricular and duodenal extracts, 250 µg, 500 µg, 1,000 µg and 2,000 µg, and of synthetic human gastrin I (SHGI), 15.75 ng, 31.5 ng, 63.0 ng and 126.0 ng, were injected intravenously through the femoral vein into the anaesthetised male rats. Injected doses of the saline solution of the proventricular extract and SHGI caused increased acid secretion whereas injected doses of solutions of the duodenal extracts produced no increase in acid secretion. The saline solution of the extract from the single duodenum was completely injected intravenously into the prepared rat at once and it produced no rise in acid secretion. The mean rates of acid secretion in µEq per 10 min to the injected doses of the stimulants were calculated according to the method of Lai¹². The log dose-response data showed parallel straight lines for the 500 µg and 1,000 µg of the saline solution of the proventricular extract and 31.5 ng and 63.0 ng of the SHGI. These doses were then used for the 2+2 (four dose) assay procedure.

The minimum dose of histamine which elicited acid secretory activity was determined on the anaesthetised rat preparation and was found to be 8 µg histamine dihydrochloride.

The crude extract from the proventriculus was 19.86 ± 2.38 mg per g wet weight of the proventriculus. When 500 µg and 1,000 µg of saline solution of the proventricular extract were injected intravenously, the mean rates of acid secretion were 2.52 ± 0.27 and 4.97 ± 0.46 µEq per 10 min (mean $\pm$ s.d. in six rats) respectively. The period of response to both the low and high doses of the SHGI and the saline solution of the proventricular extracts lasted between 30 to 40 min and the highest rate of secretion per 10 min was obtained during the second 10 min collection after the injection. The histamine content of the saline solution of the proventricular extract was 40.25 ± 2.5 ng per mg extract, using the isolated guinea pig terminal ileum¹³. The lowest dose of the saline solution which elicited acid secretory response was 62.5 µg. This dose contained about 2.5 ng histamine which was less than 1/300 of the minimum dose of histamine salt that produced acid secretory response on the rat preparation.

The gastrin activity detected in the proventricular extract was equivalent to 109.38 ± 2.5 ng SHGI per mg extract or 8.69 µg SHGI per proventriculus. The gastrin activities of the pyloric antral extracts of the following mammalian species expressed as SHGI equivalent were as follows: cattle, 55.03 ± 5.04 ng per mg crude extract; sheep, 57.1 ± 5.07 ng per mg crude extract; goat, 58.05 ± 4.55 ng per mg crude extract. The gastrin content of the chicken proventriculus per kg body weight expressed as SHGI equivalent was 3.475 µg whereas the cattle antral pyloric gastric content was 0.604 µg per kg body weight and the values for sheep and goat were 0.904 µg and 1.635 µg per kg body weight respectively. This work identifies the proventriculus as a source of gastrin in birds.

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Morphine-induced Suppression of Voluntary Alcohol Drinking in Rats

A DEBATE has arisen over whether there is a relationship between alcohol and morphine addictions. With the classical phenomena associated with addiction there is little or no evidence for a connection between the two drugs¹⁻⁴. The short-term effects are different, the withdrawal symptoms are different, morphine antagonists do not precipitate withdrawal in alcohol-dependent animals, and usually no cross-dependencies or cross-tolerances have been found, although there are reports of increased tolerance for morphine in mice that have been habituated to alcohol⁵ and in some human alcoholics⁶.

In contrast, there is evidence for a connection between the systems influencing the voluntary consumption of morphine and alcohol. In humans, it has been noted that many opiate addicts have had earlier problems with alcohol drinking, and often take large amounts of alcohol when narcotics are not available^{7,8}. In animals at least, the connection apparently has a genetic basis. A strain of rats specifically bred for high morphine consumption was found to drink more alcohol than a strain bred to ingest little morphine⁹. C57Bl mice are known to drink more alcohol than do CBA/Ca animals, and have been shown to drink more morphine solution also¹⁰. Thus animals, and perhaps humans, who are predisposed to consume higher amounts of one of the two drugs are often more likely to ingest large amounts of the other.

The present studies were designed to see if morphine itself would also affect the system regulating alcohol intake. We first tested whether a single morphine injection would influence concomitant alcohol drinking by rats that had had prolonged previous access to ethanol. Nine male Wistar-derived rats were individually housed in galvanised wire

mesh cages, under a 12 h light, 12 h dark schedule, with free access to a 10% (v/v) ethanol solution, tap water and the standard Alko food. After 141 d, five rats (mean weight=384 g) were injected intraperitoneally with 60 mg kg⁻¹ of morphine as the hydrochloride. During the 10 d prior to the morphine injection these rats drank a mean of 16.3 ml of alcohol solution per animal d⁻¹. On the first day after the injection, consumption dropped to 6.8 ml. Water drinking did not change significantly. The ratio of ethanol solution to total fluid intake, E/T, declined from 0.624 to 0.274 ($t=4.33$, 4 d.f., $P<0.02$). Alcohol drinking remained generally depressed for the first week after morphine administration. It was significantly ($P<0.02$) lower than the pre-injection level on the first, second, fifth, and seventh days after morphine was given, and only returned to the previous level 9 d afterwards. The four control rats, which were injected with a similar volume of saline, did not alter their alcohol intake.

Figure 1 shows the results of a replication of this experiment in which measurements were also taken of the daily food consumption. As previously, the alcohol intake was sharply suppressed and remained reduced for several days. In contrast, food consumption was not depressed as strongly and returned to normal more rapidly. Water drinking again was not suppressed at all.

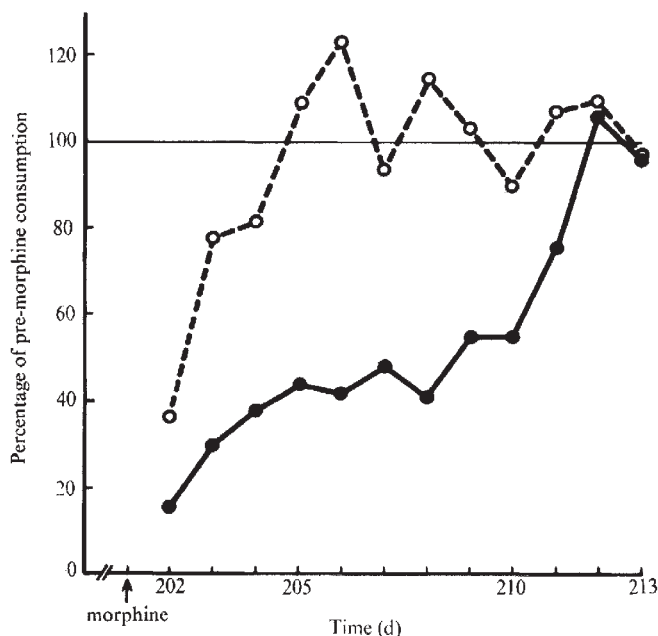


Fig. 1 Relative suppression of, ○, eating (g), ●, drinking of 10% alcohol solution (ml), after a 60 mg kg⁻¹ morphine injection. The data are from four male Wistar-derived rats, with a mean weight of 435 g at the time of the injection. They had previously had 201 d of free access to alcohol, plus food and water.

To test the specificity of the suppression to alcohol, five rats (mean weight=392 g), after 200 d of access to 10% alcohol solution and water, were given a third bottle containing a 0.013% (w/v) sodium saccharin solution for 10 d before a 60 mg kg⁻¹ morphine injection. Perhaps because of their prolonged previous experience with alcohol, the rats compensated for their saccharin consumption (11.6 ml per rat per d) primarily by decreasing their water intake from 17.7 to 5.9 ml per rat per d ($t=4.40$, 8 d.f., $P<0.005$), while alcohol intake remained nearly constant, going from 10.5 to 9.2 ml per rat per d ($t=0.67$). On the first day after the morphine injection, alcohol intake dropped to 1.4 ml per rat

($t=4.74$, 8 d.f., $P<0.005$), while water and saccharin drinking remained approximately the same as before the injection, at 6.6 ($t=0.27$) and 11.2 ($t=0.06$) ml per rat. Subsequently, alcohol consumption slowly returned to normal as in the previous experiments. The mean intakes of water and saccharin did not differ significantly from the levels observed before morphine administration. The morphine injection produced greater variability and instability in the consumption of water and saccharin. Only alcohol intake, however, was consistently suppressed.

We have also found that morphine given during a period when alcohol was withheld affects what has been called the 'alcohol-deprivation effect': if rats that have had prolonged access to alcohol are deprived of it for several days, they will temporarily drink much more alcohol than normal when access is first returned¹¹⁻¹⁶. Using conditions similar to those under which this effect has most often been observed^{12,13,16}, we administered morphine by different routes to three groups of male hooded Long-Evans rats during periods when they were without alcohol. One group (N=8, mean weight=465 g) with 81 d of access to 7% (v/v) alcohol solution was injected subcutaneously with 79.0 mg kg⁻¹ of morphine on the sixth day of an 8 d alcohol-deprivation period. The second group (N=7, mean weight=452 g) with 36 d of previous access was intubated with 10 ml kg⁻¹ body weight of 0.5 mg ml⁻¹ morphine solution on days 4, 5, 6, and 7 of an 8 d alcohol-deprivation period. The third group (N=8, mean weight=432 g) with 67 d of previous access was forced to drink a 0.5 mg ml⁻¹ morphine solution by making it the only fluid available on days 2, 3, 5, 6, 8, and 9 of a 10 d period without alcohol. When access to alcohol was returned, none of these groups showed a significant 'alcohol-deprivation effect'. In contrast, for each of these groups there was an appropriate control group, matched on previous treatment and pre-deprivation ethanol consumption level, which now showed a highly significant 'alcohol-deprivation effect'. For instance, after being forced to drink morphine during the alcohol-deprivation period, the rats' alcohol consumption changed from a mean pre-deprivation E/T of 0.524 to 0.483 ($t=0.38$). Alcohol consumption in the control group which was deprived of alcohol for the same duration, but was not given morphine, increased from E/T=0.521 to 0.852 ($t=4.77$, 7 d.f., $P<0.005$). Thus relative to the controls, the morphine groups showed a suppression of alcohol drinking. With the exception of the group intubated with morphine, to which a much smaller total dose of morphine was given, the relative suppression was of a magnitude and duration quite similar to that shown in the first three experiments in which alcohol was available when morphine was administered.

The groups receiving morphine all lost some weight as a result of the treatment. To test if this weight loss was responsible for the observed suppression of alcohol intake, three groups of male hooded Long-Evans rats were subjected to programmes of water or food restriction during alcohol-deprivation periods, such that the resulting weight losses covered the range previously shown by the groups given morphine. It was found that, unlike the morphine groups, each of these groups showed significant 'alcohol-deprivation effects', despite their weight losses.

Together these studies show that morphine, given by any of several routes, strongly suppresses alcohol consumption for over a week after its administration in at least two strains of rats, under a variety of experimental conditions. The suppression does not appear to be caused directly by the short-term effects of morphine intoxication, such as the reduction in motor activity and the inhibition of reflexes, which may interfere with drinking in the first few hours: the suppression lasts much longer than these effects, and other consumatory acts such as drinking water and saccharin solution are not suppressed. Nor is it an aversion to alcohol learned through its association with these short-term effects: the relative amount of suppression is the same when alcohol

is not present for association immediately after morphine administration. Similarly, it does not appear to be the result of alcohol being associated with morphine withdrawal sickness, in spite of a recent finding that during morphine withdrawal rats decrease their consumption of a novel saccharin solution supposedly because of such associations¹⁷. These associations tend to be formed with the most novel substance present¹⁸. In our experiment in which the rats had a choice between alcohol, saccharin and water, the alcohol was not as novel as the saccharin (210 d as against 10 d of previous access), and, therefore, if a learned aversion were the important influence, saccharin rather than alcohol consumption should have been suppressed. The weight losses following morphine administration do not seem to be responsible themselves for the suppression since similar weight losses induced by fluid or food restriction do not produce similar results. Finally, it does not appear to be caused by a loss of appetite for calories, since food consumption is not suppressed as severely or for as long as the alcohol drinking.

In summary, it appears that in spite of the difference between morphine and alcohol in terms of intoxication, tolerance and dependence, there are connections involved in the consumption of the two drugs: first, an apparent genetic correlation between predispositions to ingest them, and now a suppressive action of morphine on the drinking of alcohol. The mechanisms producing these interactions are still debatable, but the connections do exist.

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A Jumping Millipede

MOST zoologists would consider a jumping Diplopod to be a highly unlikely animal. Whilst one of us (J. G. B.) was in Sierra Leone recently, however, he collected some millipedes which can perform a succession of four or five short hops. The millipedes belong to the sub-order Stemmiuloidea. The genus represented in Sierra Leone is *Diopsiulus* Silvestri, of which there are several species, all superficially similar, which all have this characteristic jumping behaviour. Cook¹, writing on neotropical species belonging to the sub-order, reported that the animals "frequently throw themselves several inches when disturbed", and wrote that the collector of a species in Ceylon (*D. ceylonicus*) reported to Pocock that the animals were saltatory. Cook wrote that the "apparent jumping motion is caused by vigorous wriggling of the body".

The jump is an escape reaction which can be provoked by touching a millipede. The unusual feature of this behaviour is that the animal usually jumps in the same direction as it runs. It jumps 2-3 cm, lands, slides, runs forward, jumps again, lands, slides, and so on. We filmed a series of such jumps performed by *D. regressus* Silvestri at 64 and 2,000 frames s⁻¹. The sudden body movement was shown to be a rapid humping of the body about one quarter of a body length behind the head. This hump becomes a loop which is then thrown upwards and forwards, dragging the posterior half of the animal after it (Fig. 1). The head and anterior leg segments remain stationary upon the ground until they are overtaken by the rapidly moving body loop, when they are also pulled forwards and upwards.

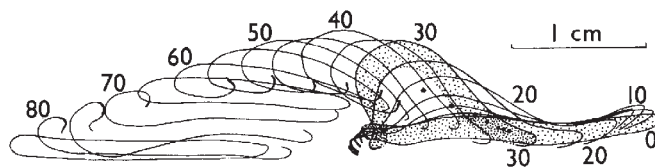


Fig. 1 Side view of a jump, from right to left, of the millipede *Diopsiulus regressus*. It is a composite based upon successive frames of a ciné film taken at 2,000 frames s⁻¹. Every tenth frame is shown, and the figures are in ms after the start of the jump. For most frames only the dorsal edge of the profile and the head and tail are shown (the head is indicated by a thickened line). The initial position (at 0 ms) and the position at take-off (30 ms) are shaded. The successive positions of the centres of gravity of each profile shown between the 0 and 30 ms positions are indicated by large black dots. They cover the acceleration distance. In this particular jump, the millipede rotates 180° about its longitudinal axis between take-off (at 30 ms) and landing (at 75 ms).

How is this jumping action produced? The stemmiuloids belong to the order Nematophora which includes the fastest running millipedes^{2,3}. A 2.3 cm long specimen of *D. regressus* can run at 5.6 cm s⁻¹, which is rapid for a millipede, a class which has specialised in slow, powerful pushing movements. When a nematophoran is running rapidly, a slight dorsal hump is seen over those legs which are moving forwards in the recovery phase⁴. This is due to the approximation of the sliding sternites by the action of powerful ventral muscles. This latter process may initiate a jump, although since the millipede halts momentarily before jumping, it must occur in a stationary animal. After the initial humping, the rapid production of a large loop is probably due to the contraction of the muscles that normally cause spiralling. The head and a few anterior leg segments remain turned forwards to maintain a grip upon the ground. (A millipede will not jump on a very smooth surface, apparently because it cannot maintain its grip.) The anterior limb of the loop is swung forwards about this fixed anterior point, and the posterior part of the body is dragged after it.

This jumping action moves the centre of gravity of the millipede rapidly forwards and upwards at an angle of about 27° (Fig. 1). In a typical jump, a 2–3 cm millipede was accelerated to a take-off speed of 48 cm s^{-1} over a distance of 0.72 cm in 0.03 s. This gave a ground level to ground level movement of the centre of gravity of about 2.5 cm (including horizontal movement during acceleration and horizontal movement during the return to ground level). The average acceleration during the jumping action was equivalent to 1.6g. Successive positions of the centre of gravity were estimated by cutting cardboard profiles of the millipede's positions at 0.005 s intervals during the jumping action. This rather crude analysis showed that accelerations fluctuated between 0 and 4g. When a smoothed distance-time curve was used as a basis for estimation, the accelerations determined were nearer the estimated 1.6g average. The maximum acceleration reached is probably not greater than about 2–3g. Since the acceleration used is relatively small for such a small animal, and the period of time available for the contraction of the muscles causing jumping is relatively large, a special catapult mechanism may not be necessary for the jump. There is a pause of only 0.03 s to 0.04 s between stimulation and jumping (that is, to frame 0 in Fig. 1). Whilst this is obviously sufficient time for the jumping reflex to take place, it is probably insufficient for much tension to be built up in the muscles antagonising the spiralling muscles (this being necessary for a catapult to operate). The millipede lands in a U-shaped position and normally falls onto its side. It usually swings the head forwards in about 0.05 s after coming to rest and then runs forward a short distance before the next jump.

The usual defensive movement shown by *Nematophora* (and, indeed, most millipedes) is a rapid spiralling of the body to enclose the head and legs. If this movement were restricted to the anterior half of the millipede, but the head and a few anterior segments remained turned forwards to maintain a grip on the substrate so that a loop formed in the vertical plane, it would produce a jump. Thus the unusual escape reaction of jumping seems to be closely related to the normal defensive reaction of spiralling.

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Similarity of Bones and Antlers gnawed by Deer to Human Artefacts

THE anthropologist, when studying excavated bone remains, often finds it difficult to distinguish specimens worked by hominids from those damaged by other processes. Although damage by carnivores and rodents is widely recognised as a potential source of confusion¹, the role of herbivores (though well known to cattle and deer herders) is less well documented. In this communication, which originates from a visit to the Hardangervidda Mountains, Norway, in 1971, I describe some bones and antlers gnawed by Norwegian reindeer and Scottish red deer. It is clear that bones and antlers so gnawed can resemble human artefacts and have sometimes been mistaken for them.

In veterinary circles the ingestion of materials other than normal food is known as pica, the abnormal chewing of bones being osteophagia. Osteophagia has been discussed by many writers, for example Blood and Henderson², who point out that in herbivores it is often a symptom of phosphorus deficiency. This habit is widespread in natural conditions and has a distinct geographical distribution which depends principally on the phosphorus content of the parent rock on which food plants are growing, but also on other factors such as excessive calcium, aluminium or iron, which can reduce the availability of phosphorus to plants. Large areas of land in many countries are, in consequence, of little value for livestock without phosphorus supplementation. This can be achieved either by dressing pasture or by supplementary feeding of bone-meal and other forms of phosphate^{3,4}, or even by the crude provision of bones for chewing. Hagerty⁵ recorded a Scottish gamekeeper leaving the legs of a red deer, which he had shot, on a hillside for other deer to chew. Fraser Darling^{6,7} has described how, even before modern advances in the understanding of animal nutrition, the inhabitants of Atlantic townships of the Hebrides ground the backbones of fish (which accumulated as a byproduct of the fishing industry) to make a meal consisting largely of calcium phosphate, which was fed to the cows during the winter. In this way the lime-rich and phosphate-poor soils of the machair were able to support cattle. This industry has now disappeared and in the 1940s cattle were often seen beachcombing for bones along the shore.



Fig. 1 Captive muntjac deer, belonging to Dr O. Dansie of Welwyn, chewing a bone which is grasped by the left cheek-teeth in a "cigar-like" manner (photo by courtesy of O. Dansie).

The chewing of bones by artiodactyls is widespread. In addition to cattle it has also been observed, for example, among red deer, reindeer, muntjac deer, camels, giraffes, wildebeest, kudu, gemsbok and sable antelopes. Rolfe (personal communication) has described a cow eating a sheep skull on the Island of Skye in 1972; MacNally⁸ has recorded cross-bred highland cattle chewing bones, and figured antlers chewed by Scottish red deer, some almost down to the basal coronet; Lindgren and Utsi (personal communication) have mentioned similar chewing by red deer in the Glenmore area so that at the end of the winter antlers have completely disappeared; Teagle⁹ has described seeing a red deer chewing a rabbit carcass in Richmond Park; and Lord Cranbrook (personal communication) observed a red deer chewing its neighbour's antlers in County Donegal in 1927.

The chewing of antlers by reindeer (and the conspecific caribou of North America) has been recorded by various

writers including Millais¹⁰, Banfield¹¹ and Kelsall^{12,13} in Canada. Mitchell (personal communication) has observed reindeer chewing shed antlers near Abismo, Sweden; and Gaare (personal communication) records that southern Norwegian reindeer not only commonly chew shed antlers but sometimes also those on other reindeer. The most detailed description of gnawing by caribou is that of Kelsall who observed that in the Bathurst Inlet area of Canada it was unusual to find any antler, even large ones, which had not been chewed. Frequently antlers were reduced to stubby butts and those frozen into snow or ice were chewed down as far as possible. He also found evidence of reindeer chewing the bleached skulls, leg bones and vertebrae of reindeer and the long bones of other mammals including wolves and long-dead Eskimos. The area of study (personal communication) is calcium rich.

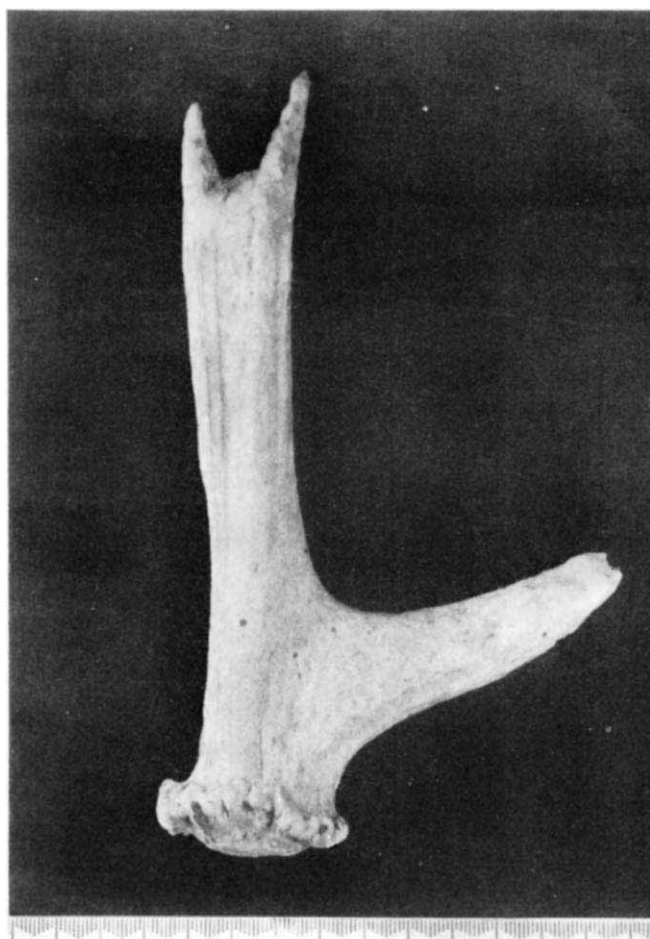


Fig. 2 Red deer antler chewed by wild Scottish red deer to create a "fork". Collected by L. MacNally (photo by courtesy of British Museum (Natural History)).

Dansie (personal communication, Fig. 1) described a captive doe muntjac deer picking up a bone in its mouth, though this was soon discarded unmarked. Cowles (personal communication) described two camels squabbling over a skeleton of a goat, already clear of flesh and skin, in the Jebel Faiyah Mountains of Oman (an area of barren rocky desert) in 1970. One camel was standing with its head in an upright position with the bones hanging out of its mouth. It was crunching the bones and swallowing the fragments. For a time both camels were grasping the remains. Evans¹⁴, Western¹⁵ and Wyatt¹⁶ have described the chewing of bones by giraffes at three localities in Kenya. The chewing of bones by wildebeest, kudu and gemsbok is known in the

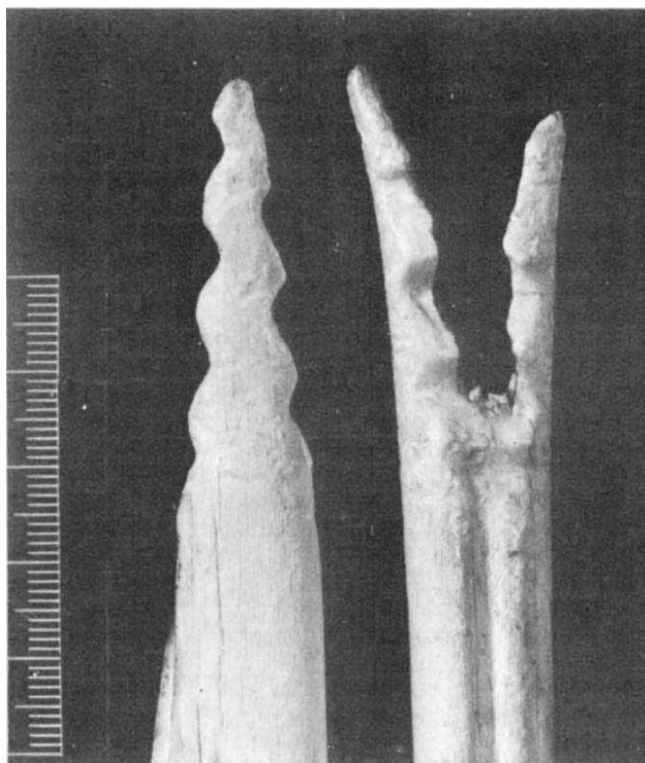


Fig. 3 Reindeer metatarsal chewed by wild Norwegian reindeer to create "fork" with zigzag margins at its proximal end. Collected by the writer near Sandhaug, Hardangervidda Mountains, Norway, 1971 (photo by courtesy of British Museum (Natural History)).

Kalahari Desert (personal communication from Yellen) and by sable antelopes in the Simba Hills, Kenya (personal communication from Leakey).

The chewing of bones by artiodactyls can lead to injury or even death. Kelsall¹² has observed in Canada that caribou may continue to chew antlers until after this has caused bleeding about the mouth. MacNally⁸ records instances of a red deer with a sheep skull and another with a pelvic bone, which they had attempted to eat, wedged around their lower jaws. In some areas of Africa, where shortage of phosphorus in plants coincides with occurrence of the anaerobic bacterium *Clostridium botulinum* type D, cattle which chew the bones of animals dead on the range may die from botulism caused by poisoning by toxins present in associated decaying musculature (Riemann¹⁷; Smith and Jones¹⁸).

Since deer, like cattle and antelopes, have no upper incisors, they are dependent on their cheek teeth for any chewing which necessitates the use of opposing teeth. The chewing of bones by red deer and reindeer has been witnessed by various observers. MacNally (personal communication) has described seeing a Scottish red deer hind chewing a bone. This was held aslant in its jaws, not crossways, with about four inches protruding at an angle behind the incisors, while it chewed on the end with its cheek teeth. After a short time the bone was transferred to the other side of the mouth and chewing was continued on the cheek teeth of that side. Wheeler (personal communication) has given a similar account of a red deer hind chewing a bone (apparently a metapodial of a sheep or deer) near Cape Wrath, Sutherland, in 1966. The bone was held lengthwise in its mouth with more than half projecting forwards and a little to one side, "like a cigar". The bone was continually shifted from side to side. Dansie's photograph of the muntjac deer (Fig. 1) shows this animal holding a bone in a like manner. Kelsall¹² has described a

similar procedure by Canadian caribou, chewing being with a sideways movement of the jaws.

Most bones and antlers are oval and not exactly circular in cross-section. In consequence, when a deer holds one with its longest dimension pointing forwards and applies a sideways chewing movement, the object becomes oriented with its widest diameter across the width of the mouth. Continued chewing will plane off the top and bottom until the marrow cavity or antler core is reached, leaving only the sides intact. The fork-like remnants which result are a characteristic product of chewing by deer. Four series of examples will be described here. (1) Deer metapodial chewed by a red deer: well developed "fork" on both ends. Since this bone is dorso-ventrally flattened at its distal end, laterally at its proximal end, the "prongs" of the two forks are not in the same plane but are set as if rotated 90°. (2) Red deer antlers chewed by red deer. Less well developed "forks", the "prongs" being at the sides of the widest diameter (see Fig. 2). (3) Reindeer metatarsal chewed by a reindeer (see Fig. 3): the "prongs" of the "fork" are at the sides of the shaft. The remarkable zigzag (a feature first described in 1957 by Kelsall) matches the alternating upper and lower cheek teeth of the chewer (see Fig. 4). The perfection of this specimen is unusual. (4) Reindeer antler tines chewed by reindeer, showing a less well developed zigzag than (3) and some shredding. Kelsall (personal communication) has described finding many hundreds of bones and antlers similarly chewed by Canadian caribou. A description by Gordon of gnawing of antlers by Canadian caribou is in preparation.

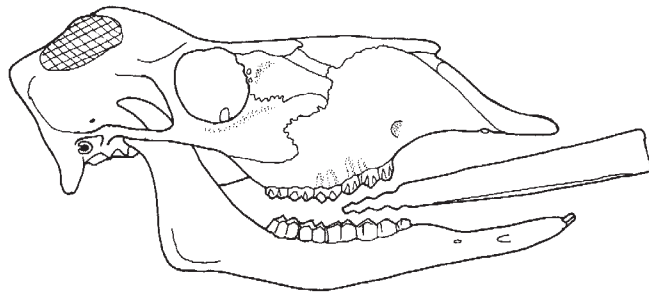


Fig. 4 Sketch showing relationship between the specimen shown in Fig. 4 and the cheek-teeth biting surfaces of a reindeer.

Since phosphorus deficiency in herbivores is unlikely to be a new phenomenon, evidence for it is to be expected in the palaeontological record. Antlers and bone remains of the pigmy deer, *Cervus cretensis*, of Pleistocene age, showing apparent deer chewing, have been collected by Sondaar from a cave west of Rethymnon, Crete (see Fig. 5). The occurrence of this phenomenon in a limestone area is in accordance with the known frequency of bone chewing by deer in calcium-rich areas today, as outlined earlier. Similar Pleistocene remains have been described from other Cretan localities by Kuss¹⁹, who attributed damaged antlers and bones of the same species of deer to an "osteokeratic culture" of Palaeolithic man. He figured fragments of antler, radius, ulna, metatarsal and mandibular ramus, all showing "forks" with "prongs" at the sides of their planes of flattening. There is some evidence of a zigzag pattern similar to that of the reindeer-gnawed specimens described earlier. The alternative interpretation, that these Cretan remains have, in fact, been chewed by deer, needs to be given consideration.

I thank L. MacNally and G. Kinns who provided antlers chewed by red deer and information; P. Sondaar who drew my attention to the Cretan problem and provided specimens; and A. W. F. Banfield, G. S. Cowles, Lord Cranbrook, O. Dansie, E. Gaare, B. C. Gordon, J. P. Kelsall,

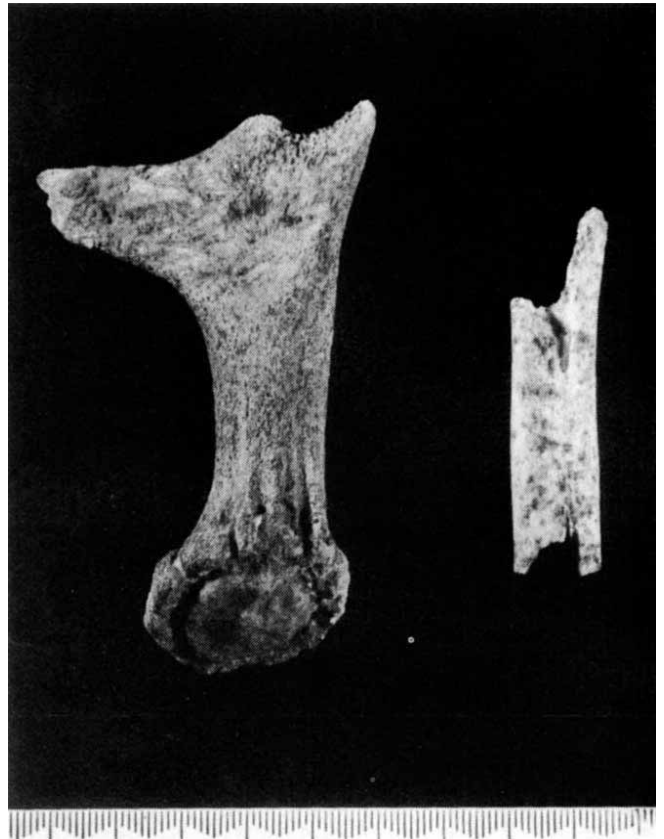


Fig. 5 Antler base and radius of the Cretan pigmy deer, *Cervus cretensis* (Pleistocene age) chewed by deer. Collected by P. Sondaar near Rethymnon, Crete (photo by courtesy of the British Museum (Natural History)).

Mrs L. S. B. Leakey, G. F. Mitchell, W. D. I. Rolfe, W. G. Teagle, E. J. Lindgren, M. N. P. Utsi, Miss E. C. Wheeler and J. Yellen who generously shared their own observations or provided information.

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BOOK REVIEWS

Einstein

Einstein: The Life and Times. By Ronald W. Clark. Pp. 670 (28 plates). (Hodder and Stoughton: London, September 1973.) £5.50.

If people who are not scientists are asked to describe their image of a scientific genius, the picture they paint seems more often than not to be based on a distorted memory of Einstein. The shaggy hair, the neglected appearance, the absentmindedness (presumably all induced by a complete devotion to scientific research) together with a general air of benevolence—this is what a real scientist should be like.

The first thing that R. W. Clark's new biography of Einstein does is to confirm that this popular representation fits the man quite well. Einstein truly paid little attention to his own appearance. His friend, Infeld, noted that Einstein always tried to minimise external distractions. "Long hair minimises the need for the barber. Socks can be done without. One leather jacket solves the coat problem for many years. Suspenders are superfluous, as are night shirts and pyjamas." Again Einstein was often absentminded: he was constantly mislaying his keys, even when taking his wife home after their wedding ceremony. Moreover, these characteristics did result from his devotion to science. The Universe came first, and everything else, including his own wife, came second. This latter circumstance was observed with some disapproval when he moved to the United States. "Your wife seems to do everything for you", observed one Princeton wife to Einstein. "Just exactly what do you do for her?" Yet the air of benevolence was also real enough. Even if the innumerable stories of helping children with their mathematics homework are apocryphal, they genuinely reflect something of the man.

This correspondence between the myth and the reality is typical of Einstein. When we move from the level of the popular image to consider Einstein as a major figure in the history of science, we find that at this level, too, he seems to provide a paradigmatic instance of the leader of a scientific revolution. If, for example, we look at the life of that other scientific genius, Newton, we can note the following points. Many of Newton's major concepts were worked out over a short period of time, while he was still a young man. In the latter part of his life, he produced no work of the same signifi-

cance. Finally, although he inaugurated a revolutionary change in science, his mode of thinking also showed a clearly conservative trend. (This was referred to by Keynes in his description of Newton as "the last of the magicians".)

If we now compare Einstein's development, we find that his major work was also concentrated into a short period of time when he was a young man. In 1905 he published papers on Brownian motion, the photoelectric effect and special relativity. All three were of fundamental significance for the evolution of physics. Einstein's name has become so inseparably linked with relativity that it is salutary to remember he was awarded the Nobel prize for his work on the photoelectric effect. Moreover, his work on Brownian motion effectively concluded a centuries-old debate on the nature of matter. At least one outstanding scientist—Wilhelm Ostwald—accepted it as the final, conclusive proof of the atomic theory.

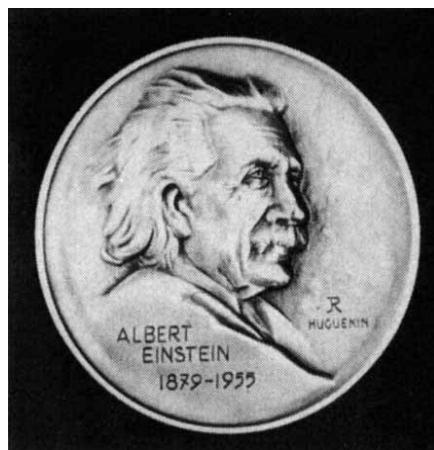
It was, however, on relativity that Einstein subsequently concentrated, leading on to the publication of the General Theory during the First World War. Yet, again like Newton, his later life produced nothing of major scientific importance. During this latter period his own particular streak of conservatism also came to the surface. His virtually single-handed fight against indeterminacy—excellently described in this book—left him isolated in the world of physics. In 1935 he told an old friend: "In Princeton they regard me as an old fool".

Even in the interplay between science and the world of politics, Einstein seems a typical twentieth-century figure. He is the persecuted Jewish intellectual fleeing from Nazi oppression. He is the well meaning scientist drawn into the problems of war (and, according to Clark, rather more knowledgeable about the atomic bomb project than has generally been supposed). Finally, he is the scientist who, after the explosion of the first atomic bombs, recoils from what has been done, and works and speaks for methods of control.

This archetypal quality of Einstein's life reflects two things: first, the essential qualities required of a genius, but, second, the way he became—in Clark's words—"the conscience of the (scientific) world". It is with these aspects of Einstein that Clark's biography is concerned, rather than with a detailed critique of Einstein's scientific ideas (though he examines the general nature of these ideas and their recent history

with distinct skill). He has provided us with a massive and fully documented account which, in its main outlines, must surely be regarded as definitive.

This does not, of course, mean that the text cannot be faulted. In terms of sources, for example, although Wertheimer is mentioned in passing, his published account of conversations with Einstein in the period leading up to the General Theory is not included in the bibliography. Again, Clark is not always accurate in his background details. Thus he implies (page 224) that a Joint Permanent Eclipse Committee was specially set up in England for the solar eclipse of 1919. In fact, as the name of the committee implies, it had been established on a continuing basis long before.



[Ronan Picture Library]

Such omissions or ambiguities are rare; nor is there a large number of misprints. The most irritating aspect of the book is the complete lack of correlation between the text and the references. The latter, which are extensive, are collected together at the end of the book, but contain no indication of the page to which they refer. The text, on the other hand, provides no mention of the references at all.

It would be wrong, however, to end a review of this excellent biography on a note of criticism. Rather I should say that Clark puts forward certain general hypotheses concerning Einstein that would repay further examination. One concerns the reason for Einstein's displacement from the mainstream of physics: according to Clark, this derived in particular from a concentration on mathematics at the expense of physical reasoning. But the most important of these themes is declared in the foreword—that Einstein was "one of the great

tragic figures of our time". (This is one of the points where the analogy between Newton and Einstein breaks down: Newton could hardly be considered in any sense as a tragic figure.) Clark develops this statement throughout the biography. If it remains not entirely convincing at the end, this does not derive from poor exposition by the author. The difficulty stems more, perhaps, from Einstein's own single-mindedness. It is hard to discern the century's great tragic hero in someone who saw the Universe as so much more important than human activities.

A. J. MEADOWS

Aspects of the Earth

Topics in Geophysics. By P. J. Smith. Pp. x+460. (Open University: Bletchley, May 1973.) £2.50.

THIS is a textbook for a short course in geophysics given by the Open University. The book does not attempt to cover the whole subject but chooses three topics as representative of different aspects.

The first is the structure and physical properties of the Earth's crust and upper mantle as determined from seismology and gravity; this represents a largely factual account about whose conclusions there is little doubt. The next topic is the heat flow from the Earth and its thermal properties. This is a subject in which a considerable range of views is possible owing to the inaccessibility of the deep interior of the Earth to temperature measurement. The third topic is earthquakes with special reference to prediction and prevention; this is a subject with engineering and social implications.

In the context of the Open University the plan of the book seems very suitable. Their short course could not cover the whole subject, except in a very superficial way, and the presentation of three contrasting topics in some detail seems a good way of providing something with a real intellectual content to students who do not intend to become professional Earth scientists. Within the three topics the author has a severe problem of choosing what to put in and what to leave out, and agreement is not to be expected. To me it seemed that in discussing the Earth's crust there was too much emphasis on gravity and too little on the mechanisms of earthquakes, the study of which has so strikingly contributed to the understanding of plate tectonics.

The figures are nearly all reproduced from papers; many of them could have been greatly improved by redrawing and some are hardly intelligible (for example, the map showing Pacific guyots and that of Japanese earthquakes).

How far the book will find an audi-

ence outside the Open University it is difficult to say. It is interesting and worth reading for a schoolboy or an undergraduate with an interest in the Earth, but have they the time to read a book that confines itself to three selected topics? I hope they have.

E. C. BULLARD

Jodrell Bank

Out of the Zenith: Jodrell Bank, 1957-1970. By Bernard Lovell. Pp. 255 (8 plates). (Oxford University: London, Melbourne and Toronto, November 1973.) £5.50.

SIR BERNARD has made the hiatus caused by conversion work on the Jodrell Bank 250-foot Mark 1 radio telescope the opportunity for an account of what he likes to call his "stewardship of that unique instrument". Since he describes not only the scientific work but also the diplomatic and managerial activities that went on behind the scenes to keep the show on the road, many interesting issues arise that can only be touched upon in a brief review.

Although for most of 1957-70 the Mark 1 was the largest fully steerable telescope in the world, it was involved directly in few of the most spectacular radio astronomical discoveries of the period—quasars, the radio source counts, pulsars. The initiative in the study of discrete sources has remained firmly with the interferometers developed in the early fifties. This is mainly because, to achieve a resolution or positional accuracy comparable with optical telescopes, radio telescopes with overall dimensions of the order of miles, rather than hundreds of feet, are needed. Indeed in one of the most successful programmes pursued at Jodrell Bank, the study of the structure of extragalactic sources, the Mark 1 dish was itself used as one arm of an interferometer, the other being a variety of much smaller aerials at ever increasing distances. By 1962, using a baseline from Jodrell Bank to Lincolnshire, Henry Palmer and his group had surpassed the best resolution obtainable with optical telescopes (1" of arc), and by 1966 they had shown that a number of quasars have sizes smaller than a few per cent of this.

Had these studies continued throughout the sixties there could now be some structure information down to 1" or better for all the brighter sources, a task now being undertaken by the new Cambridge 5 km interferometer (which will provide complete maps of the sources). Instead, Sir Bernard describes how he diverted Palmer and his group to what proved to be the chimaera of intercontinental baselines with the Russians as the other end of the interferometer arm. An intercontinental link was eventually achieved in 1969 with the huge Arecibo telescope

in Puerto Rico, but some two years after the Canadians and Americans had been on the air.

The programmes to which a telescope like the Mark 1 is ideally suited are those pursued by the groups under R. D. Davies on the 21 cm hydrogen line (primarily the structure of our own Galaxy), under R. G. Conway on the accurate fluxes, spectra and polarisation of radio sources, and under Graham Smith on the pulsars. Curiously, Sir Bernard finds space to describe only the last of these three programmes, bowing no doubt to popular interest. Instead there is an account of what might be called 'radar games' in which the United States and the Soviet Union bounce messages of dubious goodwill to each other off the Moon, balloons and what have you, using Jodrell Bank as a sort of telephone exchange. Like the manned space programme, this was no doubt entertaining for the general public, at least on the first occasion.

Jodrell Bank has been *par excellence* a pulsar observatory and has discovered more than half the northern hemisphere pulsars known. The pulsars therefore deserve their three chapters in the book, although I frankly think that Graham Smith might profitably have been asked to contribute these chapters. Indeed, a more balanced and penetrating account of Jodrell Bank's scientific work might have been arrived at by asking Palmer, Davies, Conway and Smith each to contribute sections on their own programmes, with Sir Bernard providing those interesting glimpses of the committee men in action. That, of course, would not be Sir Bernard's style, and instead we get an account of pulsars which might have been satisfactory in 1969, but seems a bit thin on the observational side for 1973. And the only theoretical work described is Gold's now superseded 1968 model. No mention at all of Pacini's 'oblique rotator', now all the rage.

There is a chapter on Sir Bernard's own observational work on red dwarf flare stars, a topic which I found interesting and not, I think, covered in other popular books on astronomy. There is a curious sentence here, though. After describing the ultimate fate of the Sun as a white dwarf, he goes on (page 170): "However, if the star is much more massive than the Sun—at least one and a half times greater mass according to present views—it is believed that the gravitational forces during the collapse are so great that the final state will be that of a neutron star". Black hole *aficionados* (and who is not one nowadays?) will know that if the mass of a star is greater than about one and a half times the mass of the Sun, not only can it not be a neutron star but *sui generis* must become a black hole, unless it sheds some of its mass.

As an account of developments in sixteen years of radio astronomy, this book would be a decidedly poor buy (you would still learn more from Graham Smith's excellent old Pelican book). Sir Bernard says that this is not his intention, but why then is there a chapter on cosmology, practically irrelevant to Jodrell Bank's work? Even as an account solely of Jodrell Bank's contribution, the book is unsatisfactory. The performance and capability of the telescopes could have been described, and compared with those at other observatories. A bibliography of Jodrell Bank publications could have been provided (though the referencing is in fact excellent). And I believe space could have been found for all the main research programmes.

The main interest of the book derives, however, from the insight it gives into how a major research establishment is run (a dictatorship, but benevolent, one hopes) and into what goes on at those committee meetings where the taxpayers' millions are carved up.

One remark did intrigue me. After describing experiments in bouncing radar signals off the Moon, Sir Bernard says (page 212): "The system was subsequently widely used for military purposes". Is that to be taken to mean that Jodrell Bank has been used by the military to detect radar signals reflected from the Moon?

M. ROWAN-ROBINSON

Marxist Science

Science and Philosophy in the Soviet Union. By Loren Graham. Pp. ix+584. (Allen Lane: London, September 1973.) £7.

DIALECTICAL materialism is too much like an established religion to be discussed dispassionately. In its name, as in the name of Christianity or of Islam, many terrible crimes have been committed and many errors promulgated. But since any philosophy of science within the Soviet Union is bound to be expressed in the vocabulary of Marx, Engels, Plekhanov and Lenin, it may also take some responsibility for the many valuable scientific discoveries and philosophical insights that have come to some of those gifted people who have sought them honestly within that framework of thought. It is a grave folly to suppose that this particular offshoot of the European philosophical tradition can be dismissed as a farrago of nonsense, on a par with Nazi racism or scientology.

Professor Graham is concerned with the intellectual conjunctions between science and official Marxism in the Soviet Union. Although not in the least a Communist apologist, he explicitly avoids extended discussion of political and social factors: for the purposes of

his book the Stalinist terror is seen as an irrational brutal fact of life, acting by *force majeure* on a sophisticated but defenceless scientific community. From this (deplorably unmarxist!) viewpoint, the Lysenko affair was a gross aberration, a bizarre and tragic pseudo-scientific interlude of no relevance to the main theme. It is more remarkable that by mixing ideological slogans with subtle technical equivocations the scientific leaders managed fairly well to ward off similar threats to scientific integrity—witness the failure of G. C. Chelintsev's attack on the resonance theory of the chemical bond, and the preservation of quantum theory against a variety of 'philosophical' objections. It could even be argued that Soviet psychology was actually strengthened by the discipline of a strict regimen of Pavlovian conditioning in place of the forbidden ecstasies of the Freudian unconscious.

Anyway, that was all about twenty years ago. Despite many serious disabilities—repression of political dissidence, arthritic administration, obstacles to travel and to the easy flow of information, uneven quality of technical facilities, and so on—Soviet science is very much the same in content as its counterparts in Europe, Japan, India, and the United States. They are under the sway of the same scientific fashions, they take off their hats to the same brilliant performers, and attempt to solve the same scientific problems with the same algebraic formulae, computer printouts, bubble chamber photographs and electron micrographs.

The question is: does the Soviet philosophical commitment to dialectical materialism make a significant difference to Soviet scientific achievements? By an admirable scholarly, intelligent and judicious appraisal of characteristic writings of many leading Russian scientists, Professor Graham provides us with precisely the material we need to make such a judgment for ourselves. He has had to cover a great deal of ground—cosmology, relativity, quantum mechanics, genetics, the origins of life, structural chemistry, cybernetics, physiology and psychology: it is a remarkable achievement to appear so much at ease, so lucid, and so reliable in such a variety of disciplines.

As a practical philosophy of science, dialectical materialism seems to have both strengths and weaknesses. The emphasis on 'materialism', for example, is so close to the naive realism of most active scientists that it contributes very little to our understanding. The attack on philosophical idealism, which was a distinguishing feature of Marxist thought in the late nineteenth century, now looks like flogging a dead horse, and is mainly used now as a form of invective against ideological opponents. In recent years the verbal allegiance to

materialism seems to have produced little more than elaborate rationalisations for the acceptability of perfectly respectable scientific concepts such as the relativistic equivalence of mass and energy or the wave-particle duality of quantum mechanics. In the field of psychology this metaphysical commitment merely adds confusion to the undecidable propositions that arise when attempts are made to define consciousness. The swing of fashion in favour of 'cybernetics' (what we now call 'systems analysis', or 'automation') in the Soviet Union in the 1960s suggests that the very word 'materialism' may predispose Russian scientists, philosophers and politicians to the 'technological fix' where more humanistic and humane methods are called for. As Professor Graham remarks "while in the 1930s it was possible to speak of the Bolshevisation of science, in the 1960s it was possible to speak of the scientisation of Bolshevism".

On the other hand, 'dialectics' is a good armour against crude scientism. Dialectical thinking, where the anti-paradigm must always be kept in mind, where the negation of the negation must be envisaged as a higher synthesis, has all the flexibility, subtlety and dynamism of the best creative speculation. In the biological realm, where non-linear cooperative phenomena are primary, the principle of the transformation of quantitative into qualitative differences is particularly valuable as an antidote to naive reductionism. Oparin's brilliant initiative on the origins of life obviously owed a great deal to this way of thought.

But the dialectical process (essentially an abstraction from the real processes of physical tension or of social conflict) is only one of many metaphors that may be applicable to the natural world or to our attempts to describe it. Since the time of Engels—the main authority on dialectical materialism as a philosophy of science—we have learnt to apply other deep and fruitful concepts such as feedback, entropy, complementarity, invariance, symmetry, connectivity, which are equally illuminating. To avail themselves of these powerful concepts many thoughtful Marxists have tried to incorporate them into dialectical materialism by showing that they are somehow subordinate to, or implicit in, the dialectical process itself: in the end, this eclecticism conflicts with the orthodox claim that the philosophy of Marx and Engels was and remains uniquely comprehensive. Official Soviet philosophy will not easily accept that the Marxist categories themselves ought to expand out of recognition, or wither away, under the impact of scientific progress.

A cardinal principle of Marxism has always been the unity of theory and

practice. This principle is mentioned by Professor Graham from time to time, but not, I feel, with adequate critical force. All too often it has been used in Russia as a crude ideological justification of some political or bureaucratic reorganisation *à la* Rothschild, without serious analysis of the underlying problems. A sad feature of the social structure of Soviet science, and of the attitude of Soviet scientists, is the gulf that still exists between the pure and the applied, the academic and the industrial, the basic and the technological. In these respects it is not obvious that the despised and corrupted institutions of capitalism are less 'relevant' than their socialist counterparts. Even within academic research there is a sharper professional distinction, and less intellectual cooperation, between theoretical and experimental physicists in the Soviet Union than in Britain or the USA. One would have more respect for dialectical materialism as a practical philosophy of science if one saw much more of this hypothetical unity in action.

Yet dialectical materialism does have another major virtue: it is a serious philosophy of nature that is genuinely favourable to scientific progress. Within its legitimate spheres, the primacy of scientific knowledge is always to be respected. Any suggestion that there could be a conflict between a scientific discovery and a theoretical deduction from metaphysical foundations is quite foreign to this philosophy, as originally propounded and as now largely practised by Soviet scientists. It is more or less beside the point whether this really means that 'dialectical materialism' is synonymous with the 'scientific viewpoint', or whether it implies no more than that "the Marxist dialectic . . . is a general orientation and culture of thought that helps each person to pose a problem with greater clarity and purpose and thereby helps him to solve the riddles of nature" (N. N. Semenov). As Professor Graham argues, we may reasonably believe Soviet scientists, and accept the evidence of their scientific achievements, when they say that they have been greatly helped in their work by their adherence to this philosophy.

The urge to interpret scientific knowledge within a consistent philosophical framework thus encourages many leading Russian scientists to consider carefully the hidden assumptions and overall structure of their subjects. In our own scientific community the absence of a tradition of serious discussion at this level of generality leaves a gap that is filled by crankiness, amateurish superficialities or ponderous logic chopping. In the Soviet Union, philosophical controversy and polemics within supposedly technical scientific disciplines may be a small price to pay

for a higher degree of general intellectual awareness. What is so depressing is that the political and ideological overtones of such important (and essentially harmless) endeavours repel many thoughtful and intelligent Russian scientists, who try desperately to keep as far away from such matters as they can, and to practise their technical skills even more narrowly than they might in a less wicked world.

All in all, this is evidently a subject of great interest and importance, and we must for long be indebted to Professor Graham for exposing this material to us in such a clear and sympathetic light. JOHN ZIMAN

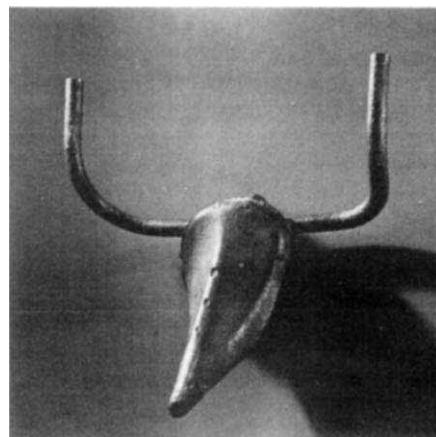
Seeing and Believing

Illusion in Nature and Art. Edited by R. L. Gregory and E. H. Gombrich. Pp. 288 (22 plates.) (Duckworth: London, October 1973.) £7.50 cloth; £3.25 paper.

THE reviewer of a symposium is tempted to write another article for the symposium instead of reviewing the articles in it. The six authors who contribute to *Illusion in Nature and Art* omit no essential aspect of the subject, and the examples they discuss are striking. Nevertheless, the temptation has not been wholly resisted.

Two of the articles are not, strictly speaking, about illusion, but about techniques for conveying information about an experience without the perceiver having personally to undergo the experience. Gombrich considers this in the broadest sense. He starts by quoting Plato's condemnation of art as an aspect of the "lower reaches of the soul", and agrees with the diagnosis but not with the condemnation. His article rambles amusingly round the subject and refers often to points made in other articles in the book. He calls attention to Houdon's technical trick for imparting a glint to the eyes in his bust of Voltaire by making a little knob of iris protrude through the cornea. He makes the interesting, and probably valid, assertion that an animal lacking the flexibility and imprecision that enable it to make mistakes could not learn. In a somewhat similar way, Penrose deals with the transmission of information by imprecise means. He stresses that painters and sculptors, like poets, expect the perceiver to do at least half the work. If gross distortion helps, he considers it justified.

The articles by Derogowski and Gregory are more immediately concerned with illusion. Every illustration, whether a line drawing or a minutely executed picture, distorts nature. Seeing it as the representation of something involves framing a hypothesis and then testing the



Picasso's *Bull's Head*, made from a bicycle saddle and handlebars, illustrated in *Illusion in Nature and Art* (design and research by Alphabet and Image).

hypothesis to see if it is tenable—as in scientific research. Children, especially those from an environment in which pictures are unknown or unusual, may not immediately comprehend even the most straightforward illustration. Deliberately ambiguous drawings give us an inkling of the nature of the difficulty. Having seen one interpretation it is sometimes difficult to capture the other. Derogowski discusses the effects of culture on this; one tends to select the interpretation that is most compatible with the familiar environment. But there is more to it than that and neither article explores the matter fully.

Escher's drawings are not reproduced in this copiously illustrated book. This is a pity because everyone I have tested sees one of his reversible staircases the same way round at first, then it 'flips' and it is difficult to recapture the first interpretation. What is the basis of immediate, but not lasting, appeal? Drawings of impossible objects, such as the well-known U-shaped trident and Penrose's triangle, are different. They are disconcerting because both forms are seen simultaneously. Blackmore discusses the physiology and neurology of vision and the conditions in which we get contradictory information. He comments interestingly on the role of binocular vision in depth perception: it is useful, but not essential, and is little use beyond about 30 m. At greater distances many other clues can be used though it may take a one-eyed person some time to learn them. A convention in painting is to use blue to emphasise distance. Many painters, from Patinir to Dali, used the technique more impressively than Turner. But it is a fuzzy picture by Turner that is reproduced.

The longest article is by Hinton on camouflage, mimicry and warning in animals and plants. It is packed with information and thoughtful suggestions. He assumes that selection is so

efficient in the long run that every characteristic is functional. The standard arguments, that the effect of the early steps towards cryptic marking would be too trivial to matter, and that beneficial effects of crypsis have seldom been demonstrated, are countered reasonably enough by the argument that the experiments have been brief and small effects require vast numbers of test organisms. He goes on to argue that a camouflaged organism is seen but not recognised, and that there is no basic distinction between being seen, and rejected as inedible, and being camouflaged. That left me unconvinced.

Eye-spots on butterfly wings probably alarm predators for long enough to permit escape, and they may divert attack to a point less vital than the insect's body. Hinton's suggestion that a chrysalis resembling a monkey's face, and a bug with its head distorted to look like a crocodile, get protection from these resemblances in spite of the difference in size between mimic and model, is intriguing. Insects have a long evolutionary history; it is therefore understandable that effects such as this should be commonest among them. Hinton extends the palaeontological argument by pointing out that a poisonous plant would gain little by using coloured flowers to warn colour-blind herbivorous mammals; he therefore invokes selection during the period when the environment was dominated by lizards with colour vision. Discussing flower colour as an insect lure, he points out that insects see in the ultraviolet; we therefore reach misleading conclusions when we consider the appearance of a flower in visible light only.

The book is handsomely got up and inevitably expensive because of the illustrations. It is a pity that a little more trouble was not taken with it. There are far too many typographical mistakes, some misnumbered pictures and an incompletely labelled graph.

N. W. PIRIE

Water

Water: A Comprehensive Treatise. Edited by Felix Franks. Vol. 2 (Water in Crystalline Hydrates; Aqueous Solutions of Simple Nonelectrolytes). Pp. xix+684. Vol. 3 (Aqueous Solutions of Simple Electrolytes). Pp. xvii+472. (Plenum: New York and London, 1973.) \$43.

In these two volumes Dr Franks has assembled an impressive array of authors and has in the main allowed them complete freedom in dealing with their allocated topics. Since most of them have already made major contributions in their field, the result is a series of expert review articles, some more idiosyncratic than others, which

together provide a mine of valuable information. Everyone working in the field of aqueous solutions should have access to these volumes and they should grace the shelves of most libraries for years to come.

Volume 2 begins with an excellent critical essay, by the editor himself, on the solvent properties of water. Chapters 2 and 3 are concerned respectively with water in stoichiometric hydrates (Falk and Knap) and with clathrates (Davidson). Both these are comprehensive and provide useful background material for an understanding of solutions. Chapter 4 (Luck) is mainly concerned with hydrogen bonding in general rather than with aqueous solutions in particular. Chapter 5 (Franks and Reid) is a valuable and detailed account of the thermodynamic properties of non-electrolyte solutions. This, together with the first two chapters of Volume 3 ("Thermodynamics of Ion Hydration", Friedman and Krishnan; "Thermodynamics of Aqueous Mixed Electrolytes", Anderson and Wood) could form the basis of an excellent text on the thermodynamics of solutions. Volume 2 continues with a factual study on the phase behaviour of aqueous solutions at high pressures (Schneider) and with a group of articles on techniques. Hasted (Chapter 7) contributes a detailed review on dielectric properties with some emphasis on experimental techniques, Blandamer and Fox review spectroscopic properties (Chapter 8) mainly concentrating on ESR evidence, and, in Chapter 9, Blandamer gives a welcome account of a rather less familiar topic, acoustic properties. Zeidler (Chapter 10) deals with NMR studies on aqueous non-electrolytes, and the volume ends with an interesting evaluation of the part played by models in the understanding of water and aqueous solutions (Ben-Naim, Chapter 11). In the course of this, some commonly held beliefs about the interpretation of partial molal heat capacities are drastically undermined.

The first chapter of Volume 3, already mentioned, is an extremely clear and complete account of the thermodynamics of ion hydration, the second a factual study of the application of thermodynamic measurements to mixed aqueous electrolytes. Hepler and Woolley (Chapter 3, "Hydration Effects and Acid-Base Equilibria") discuss the best values of thermodynamic functions for the ionisation of water, and review the reviews on acid-base equilibria. "Ionic Transport in Water and Mixed Aqueous Solutions" (Chapter 4, Kay) describes some modern conductance and transference number equipment particularly suitable for work at high pressures, and reviews work, mainly from his own laboratory, showing how solvent-solute interactions affect these

transport properties. Verrall (Chapter 5) contributes a splendidly critical account of the use of infrared spectroscopy in the study of aqueous electrolyte solutions, the related subject of Raman spectroscopic techniques and their results being entrusted to another author (Lilley, Chapter 6). Herz gives an extended account of the principles of nuclear magnetic relaxation spectroscopy and its applications to electrolyte solutions (Chapter 7). Finally, Pottel extends the theory and practice of dielectric measurements to electrolyte solutions.

No review can do justice to a work which ranges over so wide an area and is written by such authoritative authors. Possibly the editor chose rather too many. There seems little reason, for example, why the main non-thermodynamic techniques should have been cut into sections, one dealing with non-electrolyte, and the other with electrolyte solutions, as in Chapter 7, Volume 2, and Chapter 8, Volume 3. It could be argued that this type of split increases the bulk of the work and makes it more difficult for the reader without gaining anything significant. One should, however, be grateful for large mercies. These volumes with the others in the series will collectively act as a major source of information on almost all conceivable aspects of aqueous chemistry, and, individually, some of the chapters are small masterpieces. One could hardly ask for more.

H. J. V. TYRRELL

Brief Lives

Aldous Huxley: A Biography. Volume 1 1894-1939. By Sybille Bedford. Pp. xv +400 (11 plates). (Chatto and Windus (Collins): London, October 1973.) £3.95.

BIOGRAPHY has clearly established itself as the No. 1 literary art form of the present era. One has only to scan the literary pages of newspapers and periodicals or glance into the windows of booksellers' shops to notice the difference between now and, say, thirty years ago. In establishing itself as the No. 1 literary art form, biography has driven out the novel. The phenomenon is interesting, and to my mind pretty dismaying: it raises a row of interesting questions which have dismaying answers not just for novelists.

To begin with: did biography drive out the novel or was it the novel that abdicated? Then: is biography more in keeping with the general cultural climate of the times; or is the novel in keeping with the artistic climate of the times but the artistic climate itself thought less of? Has the artistic climate hived off from the general cultural climate, falling victim in its separation and remoteness to arcaneity and silliness, to shallowness and sensation?

I should say yes to all of them, I'm afraid; and cite the turning away of generally-cultured readers from the 'highbrow' novel to 'serious' biography as an illustration. The advent in the novel of what academic critics call Modernism, identifying it at its outset in the work of Henry James and at its apotheosis in that of James Joyce, say a focusing of the novel on "the interior experience". The interior experience is interesting enough; but in the absence of the exterior experience it begins to appear to the generally - cultured reader narrow and sterile and ultimately mindless. When the interior experience is taken to be a *mélange* of sense-apprehensions and instinctive responses, it is indeed mindless.

The novel of the interior experience is by definition not going to incorporate the powerful attention-holding element of narrative, and probably not even the feeble attention-holding element of 'linear thinking'. Biography, on the other hand, has to tell a story, the story of its subject's life. And while giving intimations of his interior experience, it has to trace his exterior experience—a Modern biography that didn't deal with its subject's relation to other people, to society and to external events during his lifetime would seem damned silly. Should one be surprised, then, that the generally-cultured reading public have demoted the Modern and Post-Modern novel, and the novel, *tout court*, from the No. 1 place, and installed biography there?

I was stirred to these thoughts by reading Volume 1, 1894–1939, Sybille Bedford's biography of Aldous Huxley. Mrs Bedford has great literary talent, and through everything she writes shines a sympathetic intelligence—not to say an admirably unforced professionalism as well. Had she, with the present material, written a novel about an Aldous-like character—and she would have done it excellently—it would have been reviewed, along with four or five others, to the tune of about 150 words or less: this biography will be reviewed on its own to the tune of up to 500 words or more. It makes you think.

I should add, of course, that there is currently a revival of interest in Mrs Bedford's subject, including an existentialist campus vogue for his anti-Utopian and disgust-with-life books. In the context of that fashion my own opinions are heretical. When I was an undergraduate I greatly admired *Crome Yellow* and *Antic Hay*: the way he slid intellectual dialogue into the novel was marvellous. I still admire; but re-reading the books, I find they don't wear at all well. There's an air of artificiality, of divorce from the throb of life, which makes them seem thinner, weaker. With hindsight I should say that was what, as it became more noticeable and

more obviously characteristic of the author, reduced my admiration for his later books. I regret it. I can see—and Mrs Bedford makes it very plain—that he was a highly intelligent man; a highly erudite man, easily spanning the two cultures (though not much like a scientist); and in a distant, gentle way a very nice man. But the throb of life in him seems to me to have been weak. (One could link that with vulnerability, first to disgust-with-life, and then to escape-from-life into mystical states.)

I go into all this because—another heretical notion—the quality of a biography seems to me to take something from the quality of its subject. The biography of a man in whom the throb of life is weak is likely to have, as it were, lost something before it begins. Furthermore, Mrs Bedford has to cope with most of Aldous's papers having been lost in a fire: it means that an unusual amount of the book is built from what people have told her about him or written to each other about him. That's not necessarily bad—far from it. Gossip, as C. P. Snow has remarked, lies at the heart of novel making—just think of Dostoevski's and Proust's if you doubt it. Mrs Bedford's book has exactly the flavour of close, cosy, penetrating gossip which gives it the immediacy of life. (Incidentally: because so much of the book's material comes from Aldous's wife, Maria, a strong and interesting character, the biography is almost as much hers as Aldous's.)

But I insist that now is not the time to judge the book, when it is only halfway through. Mrs Bedford's primary aim is not to "evaluate" Aldous as a writer, but to draw a portrait of him over the whole of his life; so we simply have to wait to the end. Already one can see her laying the trail, in this volume, of her own perceptions and interpretations to come. But how she will end the book remains to be seen. The story we already know—a sad, sad falling away of a brilliant mind into its own negation: a sad falling away from the outward-looking discipline of rationality to soft, inward-looking experience of hallucinogens. What can she say? It will be fascinating to see.

WILLIAM COOPER

Game of Chance

The Life Game. By Nigel Calder. Pp. 142. (BBC: London, November 1973.) £2.50.

THE theme of this book is evolution: though the author describes a wide range of biological research, it is unified by constant reference to the random events, in the form of mutations and of association of molecules and cells, which are followed by the evitable selection.

"The individuals produced by breeding are the various 'plays' or 'moves' of the gene pool which may succeed or fail." Nigel Calder, who also wrote the television programme *The Life Game*, collected much of the information in the book by interviewing the leading researchers in the fields he discusses.

He describes Darwin's theories and neo-Darwinism, and has much to say about Kimura and the neutralist opponents to Darwinism. He then gives an outline of evolution, from hypotheses about natural selection of macromolecules before life began, to Richard Leakey's finding last year in Kenya of a very old and very large human skull which may alter ideas about early *Homo*. The range is wide: Bonner's work on the genetics of nematode worms' nervous systems, Margulis's theories about the bacterial origins of organelles, and the information that geophysics can provide about the separation of the primates in Africa are all discussed. Though the first chapter seems designed more to whet the appetite than to inform systematically, the rest of the book is logically constructed.

No knowledge of biology is assumed. The function of DNA is described adequately without any reference to its chemistry, though a diagram without a legend (page 14) does not enlighten further. Work on spatial information in multicellular organisms is also bravely and quite successfully tackled. An electrophoretic gel is described as an electric racetrack, and the primaevial soup was "about a third as strong in its concentration of organic material as Knorr's Chicken Bouillon".

Although so much detail is left out, the book is very rarely misleading. In some cases it fills gaps left out by conventional genetics textbooks. Thus excellent photographs illustrate the point made that humans cannot be morphologically classified into non-overlapping races. Some confusion between gene loci and alleles seems, however, to have been the origin of the statement that "a brother and sister . . . differ genetically more than the human species differs from chimpanzees". And although both sides of most arguments are given, Cavalli-Sforza's map of human migration round the world, based on gene frequencies, is offered uncritically with no mention of genetic drift (described later) which many believe makes such reconstructions impossible.

The book is certainly an excellent source of information about modern biology for the layman; but it is so up to date (Maynard Smith's conclusions on animal conflict, *Nature*, 246, 15; 1973, are described) and so wide ranging that most students of biology will find at least an intriguing new morsel of information in it.

GILLIAN MOORE

CORRESPONDENCE

ERTS

SIR,—It was somewhat enigmatic to find published in your issue of October 19 the somewhat waspish editorial "ERTS: Technological Success, Scientific Failure?" (*Nature*, **245**, 345; 1973), together with the research communication "Burning Waste Gas in Oil Fields", by T. A. Croft of Stanford University (*Nature*, **245**, 375; 1973).

The former is an ill-considered, unconvincing and poorly argued sermon on scientific and technological priorities. The latter is a dramatic illustration of the potential of imagery produced by high altitude remote sensing techniques for stimulating important economic decisions. In this particular case the profligate waste of non-renewable natural energy resources was revealed across a large area of North Africa and elsewhere. Such observations could only be made by high altitude remote sensing techniques.

While not questioning the undoubted right of your editorialist to his own views on this subject, I would nevertheless have hoped that, in a journal of the stature of *Nature*, editorials would continue to be fair and to present both sides of complex and important issues.

In Canada, the potential economic benefits of remote sensing projects such as ERTS through natural resource assessment, budgeting, and management in such areas as glaciology, hydrology, forestry, limnology, and urban planning, to name but a few, have been recognised by the setting up of a federal interdepartmental agency, the Canada Centre for Remote Sensing (and similar Provincial Agencies), to oversee the work of remote sensing applications.

ERTS has only been in existence for just over a year, during which time a vast amount of very high quality imagery has become available. A major educational effort is needed (and in some places is in process) to provide resource managers, most of whom have been trained in traditional techniques, with the necessary interpretive tools to exploit this new and relatively inexpensive technique to the full.

Satellites are here to stay. Their potential impact on many facets of contemporary applied science and economics is immense.

Yours faithfully,

R. W. NICHOLLS

Professor of Physics,
Director, Centre for Research in
Experimental Space Science,
York University, Toronto

SIR,—Thumbs down to your editorial of October 19, "Technological Success, Scientific Failure?" on the Earth Resources Technology Satellite (ERTS). Perhaps you were deliberately being provocative to sting one or two of your readers into clarifying what many are unsure about, in which case, as one of the ERTS principal investigators, I swallow the bait.

Your question is too absolute. From my own field, the mapping of structural and lithological features of the African rift system from ERTS imagery, I can balance your editorial with a criticism of space-viewed geology made by Dr J. V. Hepworth¹ in 1969: "Space photography (and imagery) is a tool *additional* to aerial photography, geological and geophysical mapping, and is not an exclusive source of conclusive answers" (my emphasis). Of course the pressures have been there, as Dr Hepworth noted, to justify the enormous expense of space-viewing by the premature or even invalid seeking of conclusive answers. Ironically, you would now seem to be berating the ERTS project precisely because of its failure to find these answers.

"One cannot imagine why any geologist would be prepared to make a partial map of his area," you say. Yes, and the ERTS imagery shows that all geological maps, on a regional scale at least, have been partial in the sense of our failure to see, on the ground or from aerial photography, all the surface geological data. Despite some grossly exaggerated claims for ERTS, which incidentally have been strongly criticised by NASA scientists themselves², and despite some of the limitations which you yourself mention (for example, the 0930 routine), the imagery provides valuable new scientific information. For the geologist, this has enabled a revision or precision of regional structure, lithological boundaries and even of the geographical base-maps—many river courses and lake shore-lines in eastern Africa, for example, are for the first time known accurately from the ERTS imagery.

Let other scientists speak for other disciplines, but the benefits of ERTS to geology, as of Lunar Orbiters to selenology, are such that one geologist would like to say, "Thank you, NASA."

Yours faithfully,

P. A. MOHR

Smithsonian Institution,
Astrophysical Observatory,
60, Garden Street,
Cambridge, Massachusetts 02138

¹ Falcon, N. L., Gass, I. G., Girdler, R. W., and Laughton, A. S., *Phil. Trans. R. Soc. A*, **267**, 40 (1970).

² Freden, S. C., and Mercanti, E. P., *Symposium on Significant Results obtained from Earth Resources Technology Satellite-1*, 3 (Discipline summary reports, NASA X-650-73-155, 1973).

Whiter than White

SIR,—Amongst the Short Notes (*Nature*, **245**, 404; 1973), the paragraph "Whiter than White" asks if the soap industry has been living in an unreal world because the use of soap per person in Britain has only increased linearly and not exponentially.

Exponential increase is best illustrated in its biological form, where any increase in population is itself capable of reproduction and there is no intrinsic limit to the increase. When increasing areas of the body receive the attention of soap, or rates increase, both areas and frequency of application have finite limits. Similarly, the washing of garments or number of eating utensils used per person must tend to a definite limit and show linear characteristics for a time. I suspect, however, that the data we have are incomplete because a linear increase means that no soap has been used much before 1825.

As a manufacturing company, our interest lies in studying the various opportunities inherent in different growth rates. When the use of a product has a growth rate of only 0.76% per annum, as shown for soap, a proper segmentation of the demand is necessary. By this is meant that specialised demand sectors, such as stainless steel razor blades, enzyme washing powders or nuclear steam boilers can have very large growth rates within an overall small increase in total demand for razor blades, soap or boilers. It is in the early recognition and in the timely development of products for these growth sectors that industries can consolidate, establish or challenge market leadership with the chance of high rewards. As seen from their annual report, Procter and Gamble have made excellent use of their experience in this by a return on capital of over 30%.

May I be permitted to re-phrase your question? Has the scientific community lived in an unreal world for the past hundred years? Can we hope that this evidence of *Nature's* study of annual reports inaugurates a new era of a symbiotic cross fertilisation between industry and science?

Yours faithfully,

W. HILL

Director and Marketing Manager,
Group Headquarters Division,
Clarke Chapman-John Thompson Ltd,
Gateshead, Co. Durham NE8 3HS

Paperback Polymorphism

SIR,—D. F. Owen comments (*Nature*, 245, 343) on the variety of colours in which R. A. Fisher's *The Genetical Theory of Natural Selection* is available in paperback, and speculates that Fisher "... would have been delighted to hear that the polymorphism in the paperback edition of his classic work had possibly affected its sales".

I happened to be with Fisher when he opened the parcel containing the first copies of the paperback edition that he had seen: he took one look at the cover (red) and said "Looks like a book by Hogben."

Yours faithfully,

A. W. F. EDWARDS

Gonville and Caius College,
Cambridge CB2 1TA

Announcements

University News

Heriot-Watt University has conferred the title of Professor Emeritus upon Dr Walter G. Green.

The University of St Andrews has appointed Professor Vincent Bruce Proudfoot to the newly established Chair of Geography.

Appointments

Mr Alan V. Holden has been appointed Officer in Charge of the Freshwater Fisheries Laboratory of the Department of Agriculture and Fisheries for Scotland at Pitlochry.

Professor J. Gani, Director of a combined school of the Universities of Sheffield and Manchester, has been appointed Chief of

the CSIRO Division of Mathematical Statistics.

Dr G. F. Humphrey, Officer in Charge of the CSIRO Marine Biochemistry Unit, has been elected Chairman of UNESCO's Intergovernmental Oceanographic Commission.

Miscellaneous

The Zoological Society of London has awarded the first British Zoologist's Medal to Sir Julian Huxley.

A British Society for Population Studies has been set up to continue and expand the scientific study of population questions. The chairman is Professor D. V. Glass of the London School of Economics and Political Science, Houghton Street, Aldwych, London, WC2A 2AE.

The 1973 Soviet State Prizes for science were awarded as follows: to Nikolai A. Borisevich and Viktor G. Vereshchagin for studies of radiation scatter by dispersion systems and the development of a new wide-range infrared filter; to Marat I. Adamovich, Boris B. Govorkov, Anatolii S. Belousov, Andrei I. Lebedev, Sergei P. Kharlamov, Evgenii I. Tamm, Academician Anatolii A. Logunov, Lev D. Solov'ev, Aleksandr M. Baldin and Al'bert N. Tavkhelidze for their work on the photogeneration of π -mesons on nucleons; to Nikolai P. Korneichuk for his work on extremal problems in approximation theory; to Academician Boris P. Nikol'skii, Professor Mikhail M. Shul'ts, Anatolii I. Parfenov, Anatolii A. Belyustin, Vladimir A. Dolidze, Archil N. Khutsishvili, Anatolii S. Benevol'skii, Viktor P. Yukhnovskii, Valentin V. Aleksandrov, Nikolai A. Fedotov and Nikolai A. Izmailov for work on the electrode properties of glasses; to Nikolai G. Rambidi, Yurii S. Ezhov, Petr A.

Akishin, Evgenii Z. Zasorin, Viktor P. Spiridonov and Mikhail I. Vinogradov for developing a new method of high-temperature gas electronography and applying it to study the structure of inorganic molecules at temperatures above 2,500° C; to Aleksei D. Shcheglov, Deputy Minister of Geology of the USSR, for publications on regional metallogeny; to Academician Innokentii P. Gerasimov, Nikolai F. Leont'ev, Yakob G. Mashbin, Dmitrii A. Lilienberg, Feofan F. Davitaya, Vladimir G. Brugger, Anatolii G. Voronov, Aleksandr M. Ryabchikov, and to Antonio Nuñez-Jimenez, Pedro Cañas-Abril, Salvador Massip-y-Valdes and Juan Torrente del Valle for work in compiling the National Atlas of Cuba; to David M. Golub for work on the development of the nervous system and the theoretical and experimental basis for establishing new nerve links and centres; and to Academician Aleksei P. Okladnikov and Viktor I. Shunkov for their five-volume *History of Siberia*.

Solutions to Chess Problems

Problem 1: 1. Q-K2 mate.

Problem 2: 1. Kt-Kt6 mate.

Problem 3: 1. Castles (QR), any; 2. R-Q8 mate. The point of this problem is that Black could have reached the position in the diagram only by moving his king or rook on the immediately preceding move. Thus, although White can legally castle on his own move, Black has forfeited the right to castle on his move (which would otherwise have been a complete resource against the threat of mate).

Problem 4: White's last move must have been K (from K3) x P (Q3) giving discovered check. Black's preceding move to this must have been P (K5) x P e.p. (Q6) giving double check, and White's move before this P (Q2)-Q4. Black's move before this must have been a bishop move to QB4 giving check to the White king on K6. The main 'try' for White's last move in the position in the diagram is K (from Q2)-Q3 giving discovered check, and Black's preceding move being P (QB7) x White piece (QN8)=Kt giving double check. This solution, however, leads to an 'illegal' position as there are too many black pawn captures of white pieces (and pawns) needed to reach it from the pawn formation of the starting position of a game of chess.

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